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THE PSYCHOLOGICAL REVIEW

THE REPLY OF A PHYSIOLOGIST TO PSYCHOLOGISTS

BY I. P. PAVLOV

Leningrad

I

The article by Edwin R. Guthrie, 'Conditioning as a principle of learning,'[1] provides, it seems to me, special interest from its cardinal tendency, altogether justified to my mind, of ascribing phenomena designated as psychological activity to physiological facts, *i.e.*, of uniting or identifying the physiological with the psychological, the subjective with the objective, which I am convinced comprises the most important present-day scientific undertaking. The author treats the principle of learning in a general way, giving the characteristics of that process in an enumeration of its fundamental features, while he makes use of the material of psychologists, or of our physiological facts obtained with the method of conditioned reflexes with animals, without distinction. So far psychologist and physiologist proceed side by side. But beyond this point sharp differences arise between us. The psychologist takes conditioning as the principle of learning, and accepting this principle as not subject to further analysis, not requiring ultimate investigation, he endeavors to apply it to everything and to explain all the individual features of learning as one and the same process. For this purpose he takes one physiological fact and in a decisive way gives it a specific meaning in the interpretation of certain concrete facts of the learning process and does not seek an actual confirmation of that

[1] E. R. Guthrie, PSYCHOL. REV., 1930, 37, 412–428.

meaning. From this, the physiologist is inclined to think that the psychologist, recently split off from the philosopher, has not yet altogether renounced partiality toward the philosophical method of deduction from pure logical work, without verifying every step of thought through agreement with actual fact. The physiologist proceeds in quite the opposite way. At every phase of his investigation he endeavors to analyze the phenomena individually and in connection with facts, determining as much as is possible of the conditions for their existence, not trusting to mere deduction or to a single hypothesis. And this I shall prove in regard to several points, in which the author opposes me.

Conditioning, association by contiguity in time, conditioned reflexes, even if they serve as the factual point of departure of our investigations, are none the less subject to further analysis. We have before us an important question: What elementary properties of brain-mass form the basis of this fact? We have not yet reached a final solution of this question, but significant data are afforded by the following experiments. With our experimental animal, the dog, if the external agent which we wish to use as conditioned stimulus is applied after the beginning of the unconditioned stimulus, the conditioned reflex occurs (according to the latest and most exact experiments possible of Dr. N. V. Vinogradov), but is insignificant and temporary, disappearing in every case if the period of procedure be continued. A durable and lasting conditioned reflex, as we have long known, is obtained only when the external agent invariably precedes the unconditioned reflex. Thus the first procedure possesses a double effect: at first, temporarily, it assists in the formation of the conditioned reflex, and then destroys it. This latter action of the unconditioned stimulus comes out clearly in the following type of experiment. A conditioned stimulus which becomes effective by means of the second (the usual) procedure,—if afterwards it be systematically applied after the beginning of the unconditioned (or becomes submerged by the unconditioned, according to our usual laboratory terminology), particularly if it belong to the category of weak conditioned stimuli,—gradu-

ally loses all positive action and finally is even converted into an inhibiting stimulus. Evidently in this case the mechanism of negative induction (in our old terminology called *mechanism of external inhibition*) gradually prevails; *i.e.*, the cell excited by the conditioned stimulus is inhibited or comes to an inhibited state with repeated concentration on the part of the unconditioned stimulus—and the conditioned stimulus in this way meets in its cell a permanent state of inhibition. But this brings out the fact that the conditioning agent becomes inhibitory, that is, on being applied alone it now calls forth in its own cortical cell not an excitatory but an inhibitory process. Consequently during the usual process of formation of a stable conditioned reflex, the passage of a wave of excitation from the corresponding cortical cell to the center of concentration of the unconditioned stimulus is exactly the fundamental condition which fixes the path from one point to another,—more or less of a permanent joining together of the two nervous centers.

Let us pass now to other features of conditioned activity, where the author instead of our diversified analysis of facts offers his own monophasic interpretation of the phenomena which take place. The delayed or postponed conditioned effect, according to our experiments, is based on special inhibition of early phases of the conditioned stimulus, since they do not fit in closely with the time of appearance of the unconditioned stimulus. The author for some reason asserts that we attribute this to 'mysterious latencies' in the nervous system, and he gives his own explanation of the facts. He assumes that when for example the sound of a bell is presented as a conditioned stimulus, the animal responds with a reaction of 'getting set to listen,' a complex motor act, and the centripetal impulses in that act are, strictly, the true stimulators of the conditioned effect,—in our case of the conditioned food reflex, the salivary secretion.

According to the author, "when the salivary glands begin to secrete, the accompanying stimuli are not furnished by the bell but by these responses to the bell. The direct response to the bell is probably over in a small fraction of a second."

And further he says, "the apparent separation in time of a conditioning stimulus and its response is then quite possibly an illusion." The author even says that "Pavlov tends to forget," in his explanation of the delay, the existence of the above-mentioned centripetal impulses from the motor apparatus. On page 360 of my book, 'Conditioned Reflexes, an Investigation of the Physiological Activity of the Cerebral Cortex,' one can see that I not only take into account the centripetal impulses from skeletal musculature, but also consider as more than likely their existence in all tissues, to say nothing of individual organs. To my mind the whole organism with all its component parts can make itself known to the cerebral hemispheres. This shows that the point is not overlooked by me, but that actualities provide not the slightest ground for accepting the fact in the way it is explained by the author.

First of all, if we grant with the author that not the bell, but the centripetal flow of impulses from the motor act of listening is the true stimulus for the conditioned effect, why does that effect, in the case of delayed reflexes, nevertheless come out, not at once, but after an interval—and (furthermore) in accordance with the length of the interval between the beginning of the stimulus and the beginning of the unconditioned reflex? For, when the unconditioned stimulus is delayed for a very short time (only a few seconds) after the beginning of the conditioned, the effect, brought about according to the author by centripetal impulses from the motor act of listening, appears as soon as 2 or 3 seconds. Where then is the explanation of the length of the delay? and how, when the conditioned stimuli precede the unconditioned by several minutes, do the stimuli of the author, the centripetal impulses of motion, act after the lapse of minutes?

But as a matter of fact there is absolutely no ground for accepting a continuous action of the stimuli of which the author speaks. The listening response as a general orienting or investigating reflex, as I have termed it, appears with every new set of vibrations which habitually play upon the animal, and usually remains in existence only for the short period of

the first application of the new recurring stimuli. Upon the formation of a conditioned reflex with a more or less short interval between conditioned and unconditioned stimuli, it is quickly replaced by the special motor reflex peculiar to the given unconditioned stimulus. And further, only the conditioned motor effect is permanently to be had, free from all trace of an orienting reaction. And thereupon the conditioned stimulus appears as a pure substitute for the unconditioned stimulus. In the case of a conditioned alimentary reflex the animal may lick the electric lamp, or appear to take the air into its mouth, or to eat the sound,—that is, licking his lips and making the noise of chewing with his teeth as though it were a matter of having the food itself. The same thing occurs in the delayed reflex which is worked out. The animal remains completely indifferent and quiet in the first period of action of the conditioned stimulus; or even (as is not seldom the case) immediately upon the beginning of that stimulus, he drops into a drowsy and sometimes abruptly into a sleeping state, with relaxation of the musculature and snoring. This, on entering the second period of the conditioned stimulus, just a little before the addition of the unconditioned stimulus, is replaced (sometimes with a start) by a clearly suitable conditioned motor reaction. In both cases it is only during the general somnolence of the animal in the course of the experiment and occasionally at the first moment of stimulation, that the orientation reaction shows itself.

And finally, on analysis the delayed reaction actually proves to be the result of interference of a special inhibition which is by itself well known to us, and is studied in detail in many cases of its appearance;—but this is not a 'mysterious latency.' The meaning of all this is clear. Although prolonged for a significant length of time, the conditioned stimulus remains one and the same; but for the central nervous system (and it is especially necessary to think of the cerebral hemispheres) it is distinctly different in different periods of its course. This comes out particularly clearly with olfactory stimuli, which we sense at first very keenly, and then quickly as weaker and weaker, even if they remain objectively con-

stant. Obviously the state of the stimulated cortical cell under the influence of an external stimulus undergoes successive changes and in the case of delayed reflexes only the state of the cell near the time of the addition of the unconditioned reflex acts as a signal for the conditioned stimulus. This is exactly the case when, from different intensities of one and the same external stimulus, we can form different conditioned stimuli,—sometimes positive, sometimes negative, sometimes linked with different unconditioned stimuli. The analyzed fact of delay is an obviously interesting case of special adaptation, in order that the conditioned reflex might not occur prematurely, so that energy beyond the necessary measure is not uselessly expended. That this explanation corresponds to reality is proved by facts. First of all it is clear from the process of formation of the delayed reflex. If the conditioned reflex be formed first with the short interval of a few seconds between the beginning of the conditioned and of the unconditioned stimuli, and then suddenly that interval be increased to a few minutes,—then the conditioned effect, hitherto quick to come forth, will promptly go on to gradual but utter disappearance. And then, on continuing the experiment, there appears for a considerable lapse of time a period of absence of all conditioned effects. Only then does the conditioned reflex appear anew, at first just at the moment preceding the addition of the unconditioned stimulus. Thence it grows gradually and recedes somewhat from the time of appearance of the unconditioned stimulus.

That the first period of the delayed reflex is indeed a period of inhibition is proved by a series of facts. In the first place, inhibition of the delayed reflex can easily be summated. And again, from the delayed reflex one can observe successive inhibition. And finally, the drowsy and sleepy state which comes out in some animals in the first part of the delayed reflex is a striking expression of the state of inhibition.

The next phenomenon, extinction of the conditioned reflex, the author also discusses without paying any attention to the details of the facts of our investigation, having in view again the very same factor conjectured by him, but no more exactly

defined than before. And along with it he now ascribes to me, besides the previously mentioned 'tendency to forget,' a concealing of something from myself.

First of all the author takes a stand against us by saying that it is not the brevity of the interval between repetitions of the non-reinforced conditioned reflexes that contributes to extinction of these reflexes, but the number of repetitions. But this is absolutely untrue. An unreinforced conditioned reflex without any repetitions, but simply prolonged from 3 to 6 minutes, ends in every case in extinction to an absolute zero —as we say, in an uninterrupted extinction, in contrast to an intermittent one. And furthermore, the author arbitrarily supposes that extinction is not a constant fact, but an exception to the rule of frequency. Again an absolutely incorrect statement. Extinction is one of the constant facts of the physiology of conditioned reflexes. Having accepted both these things in spite of reality, the author, so to speak, clears the field of action for himself and imagines some different kind of agents,—no better determined,—which in addition to the fundamental unconditioned stimulus take part in the formation of the conditioned effect. Probably here also movements of the animal are assumed, because mention is made of continuous and of various movements of the animal during the experiment. Thus, according to the author, the sum of the agents determining the conditioned reflex continually fluctuates, appearing now greater, now less. When these agents become fewer and the conditioned reflex is absent or becomes diminished, the rest of the agents, also unknown, become inhibitory as well, or, what is practically the same, they become stimulators of other responses.

The breaking up of extinction by extraneous stimuli the author explains by saying that those stimuli 'disorganize posture and orientation' which appear as inhibitors of the conditioned reflex at this stage of extinction and thus temporarily restore the reflex which was becoming extinguished.

The author does not deem it necessary to inform us, even hypothetically, just what sort of stimuli support the conditioned reflex together with the unconditioned, and what other

sort here present serve as inhibitors of that effect. When the author in his own way explains the breaking up of extinction by extraneous stimuli, why does he not state in what way the extraneous stimuli, which sweep away the action of the agents that inhibit the conditioned effect, fail to remove also the action of those which facilitate the conditioned response? For they are different stimuli from these latter.

And so there is introduced by the author, without any factual confirmation of their actual meaning, a number of unknown stimulating agents utterly undetermined in a more exact way.

We must conclude that the author understands them to be just these same kinesthetic stimuli, but arising from different muscles. Of course there are many skeletal muscles, and from them arise during their action an almost countless number of combinations, and from all of them special centripetal impulses are constantly being sent to the central nervous system. But in the first place, the most important part of these impulses proceed to the lower divisions of the brain, and secondly, under usual circumstances they absolutely do not make themselves known to the cerebral hemispheres but serve only for the self-regulation and greater precision of movements, such as, for example, the continuously occurring cardiac and respiratory movements.

Under the conditions of our experiments only such movements are reckoned with as exert an influence over our conditioned reflexes,—only those movements which form special motor reflexes, the chief and almost the only one of which is the orientation reflex to the vibrations of the immediate environment, and sometimes also defense against some chance nocuous influence on the animal during his movements on the experimental stand,—a blow from something, some sort of pinch, etc.

If the centripetal impulses, such as the author assumes to arise from all movements which we execute, really proceeded to any considerable extent into the cerebral hemispheres, then by their very number they would provide a tremendous hindrance to a relationship of the cortex with the external

world, almost excluding it from its principal rôle. Can it be that, when we talk, read, write, and in general think, our movements, which arise inevitably as we do so, disturb us to such an extent? Can it be that all this is ideally performed only during our periods of absolute immobility?

The constant fact of extinction is not due to the play of chance movements of the animal which are reflected in the work of the hemispheres, but it is the manifestation, according to law, of the most important properties of the cortical cells, as the most reactive of all cells of the organism, when they remain at work for a greater or less period of time—even if generally a short one—without a satisfying accompaniment for the fundamental innate reflexes; for, the chief physiological rôle of excitation of these cells is to serve as signals in place of the special stimuli of the latter reflexes. As the most reactive cells, they quickly become fatigued from work and go on not to an inactive state but to inhibition, which probably not only assists in their rest but also hastens their recovery. But when they are accompanied by unconditioned stimuli, then these stimuli—as we have seen at the beginning of the article—at once, and so to speak by way of protection, inhibit them and thus contribute to their recovery.

That extinction is actually inhibition, is proved as well by its successive inhibitive effect on other positive conditioned reflexes as by the transition to drowsy and sleeping states, which is without doubt inhibition.

As to the two other points, where the author offers merely his own view in place of our explanation, I can be more summary. In regard to the fact of gradual intensification of the conditioned effect during the process of its formation, it is necessary to state that in this case it is the gradual removal of extraneous stimuli which disturbs the formation of the reflex, and not the opposite, namely,—the author's view, which consists in attributing to these stimuli an ever-growing rôle in creating the conditions for the effect. During our first experiments often 50 to 100 or more repetitions of the procedure were required in order to develop a complete conditioned reflex, but now 10 to 20 times are sufficient, and often much fewer. Un-

der the present technique of our experimentation, during the first application of a new indifferent agent (the future conditioned stimulus) there results only an orienting reflex, the detection of the motor component of which in the great majority of cases rapidly diminishes to complete disappearance,—so that there is here absolutely nothing out of which this ever-growing sum of determinations of the conditioned effect should form itself, as the author puts it. It is clear that the whole process consists in an ever-growing concentration of stimuli and then, perhaps, in gradually beating a path between connected centers in the central nervous system.

Finally, in regard to the independent acquisition of a conditioned effect from the stimuli in the neighborhood, or near that to which the conditioned reflex is specifically formed, the author is again of a different mind from us. According to us, this is an irradiation of stimulation spreading over a definite part of the cortex. But the author, having taken for granted that for the conditioned stimulus there appears not merely a specific exciting agent but an orientation reflex accompanying it, now explains the matter by saying that all the neighboring agents receive their own activity thanks to one and the same orientation reflex. But this quite contradicts the facts. The neighboring agents in the majority of cases give the conditioned effect directly, without any trace of orientation. But when the orientation reflex exists besides, it is just then (on the contrary) that the conditioned effect either is completely absent or appears very weak; it comes forth and grows only in proportion to the disappearance of the orientation reflex.

And so throughout his article the author remains true to himself, to his own habits of deduction. Making incorrect use of one physiological principle—the fact of conditioning—all the details of conditioned nervous activity which he utilizes for his theme of learning he derives therefrom immediately and constantly, while a whole mass of concrete facts remain without the slightest attention on his part.

II

It seems to me that the second article, 'Basic neural mechanisms in behavior,'[2] to which I turn now, bears to a considerable extent the same tenacious character in the development of its theme as the first. This article presents a paper read by K. S. Lashley at the last International Congress of Psychology in the United States in 1929. Granting that its material is almost exclusively physiological, yet the author's method of treating it is quite that of the preceding article. The material is sacrificed to the fundamental preconceived tendency to demonstrate that the reflex theory 'is now becoming an obstacle rather than a help to progress' in the study of cerebral function, and that, of more strength and significance than the reflex theory, is for example the statement of C. Spearman, that "intelligence is a function of some undifferentiated nervous energy,"—an analogy to the tissue of sponges or hydroids, which, being crushed and sifted through bolting cloth, afterwards when settled out or centrifuged down, forms itself anew into a mature specimen with characteristic structure.

First of all I must state in a general and all-inclusive way, without going into detail for the present, that such a merciless judgment of the reflex theory divorces itself from actual facts absolutely. One may even say, it is somewhat strange that it does not desire to call attention to them. Is it possible that the author ventures to intimate that my 30 years' work, continuing still with success, with many collaborators, proceeding under the guiding influence of the conception of reflexes, presents only a drag on the interpretation of cerebral function? No; no one has the right to say that. We have established a series of important principles of the normal activity of the higher divisions of the brain, defined a series of conditions both of its waking and of its sleeping states; we have made clear the mechanism of normal sleep and hypnosis; we have produced experimentally pathological conditions of this neural level, and found means to bring it back to the normal. The activity of this level, as we have already learned,

[2] K. S. Lashley, PSYCHOL. REV., 1930, 37, 1–24.

found and is finding in itself quite a number of analogies with the phenomena of our subjective world, as is brought out in statements not infrequently made by neuropsychiatrists, educators, experimental psychologists, and in the assertions of academic psychologists.

Now, before the physiology of this neural level lies a vast horizon, with questions jutting out, absolutely definite problems for further experimentation, in place of very nearly a blind alley, in which this physiology unquestionably found itself a few decades back. And all this thanks to the use of experiments made on this part of the brain under the concept of reflexes.

Of what does the concept of a reflex consist?

The theory of reflex activity finds its support in three fundamental principles of exact scientific investigation: in the first place, the principle of *determinism*, *i.e.*, an impulse, appropriate conditions, or a cause for every given action or effect; secondly, the principle of *analysis and synthesis*, *i.e.*, the initial decomposition of the whole into its parts or units, and then the gradual reconstruction of the whole from these units or elements; finally, in the third place, the principle of *structure*, *i.e.*, the distribution of the activity of force in space, the adaptation of function to structure. Therefore it is impossible that the death sentence for the reflex theory be taken otherwise than as a misunderstanding or bias.

We have before us living organisms, man included, producing a series of activities, manifestations of force. And there is an immediate impression, hard to surmount, of some voluntary freedom of action, of some spontaneity. In the case of man, as an organism, this impression appeals to almost every one as obvious, and an assertion to the contrary seems absurd. Although Leukippus of Miletus [3] announced that there is no motion without cause and that everything arises out of necessity, is it not still being said, even of animal organisms besides man, that spontaneously active forces exist in the organism? And in regard to man, do we not hear even

[3] I take this information from Professor Kannabich's book on History of Psychiatry.

now of freedom of the will, and is there not rooted in the mass of intellects the conviction that we possess something which is not subject to determinism? I have met constantly and am still meeting not a few educated and intelligent people who are in no way able to understand how it could ever be possible to learn all about the behavior (for example) of a dog in a purely objective way,—*i.e.*, by merely comparing the stimuli acting upon the animal with the reactions to them, and therefore not taking into consideration their subjective world, which is supposed to exist analogous to our own. Of course we refer here not to the temporary, let alone immense, difficulty in experimentation, but to an absolute impossibility of complete determinism as a principle. It stands to reason that this same view is held, only with far greater conviction, in regard to man. It would not be a great error on my part if I held it probable that this conviction persists also among psychologists, masked by assertions of the *unique features of psychic phenomena*, under which, disguised by various scientifically decent synonyms, is felt all this dualism and animism immediately shared by a mass of thinking, not to say religiously minded people.

Now, just at its very first appearance, the theory of reflexes constantly increases without cessation the number of phenomena in the organism which are connected with the conditions that determine them; *i.e.*, this theory makes more and more clear the integrated activity in the organism. How can it possibly be an obstacle to the progress of studying the organism in general and the cerebral functions in particular?

Further, the organism consists of a great mass of separate parts and of billions of cellular elements, providing a corresponding quantity of separate phenomena, but closely interwoven among themselves and organized for the integrated work of the organism. The theory of reflexes divides this general activity of the organism into separate activities, connecting them with internal as well as external influences, and then unites them anew, one to another, which brings us to a more and more clear understanding of the total activity of the organism, as well as of the interaction of the organism with surrounding conditions. How has the reflex theory been

and at the present time can it be superfluous or irrelevant, since there is still neither sufficient knowledge of the connections of the separate parts of the organism, nor a more complete interpretation of all the relationships of the organism with surrounding conditions? But all internal as well as external relationships in higher organisms are above all accomplished by means of the nervous system.

Finally, if a chemist, analyzing and synthesizing, for the ultimate understanding of the work of the molecule, has to use his own imagination about its invisible structure, if a physicist, similarly analyzing and synthesizing, for a clear idea of the work of the atom, also pictures to himself the structure of the atom,—how is it possible to repudiate the structure of visible masses and take for granted some kind of contradiction between structure and function? The function of the connections, of internal as well as external relationships in the organism, is realized in the nervous system, which represents a visible apparatus. In that apparatus of course spring up the dynamic phenomena, which must be timed exactly to the finest detail of the apparatus.

The theory of reflexes began to investigate the activity of this apparatus with the definition of its special function, naturally of its more simple, grosser parts, and determined the general tendency of the dynamic phenomena arising in it. Here is the general and basic scheme of a reflex: receptor apparatus, afferent nerve, central station (centers), and efferent nerve with the tissue through which it operates. Then came and still comes a detailed elaboration of these parts. Of course a most complex and immense work has been in store and is yet in store for the central nervous stations, and in the parts of the central stations for its grey matter, and in the grey matter for the cortex of the cerebral hemispheres. This work concerns both the visible structure itself and the dynamic phenomena arising in it, while all the time of course the necessary tie between structure and function is not lost sight of. Owing to the difference of method in the study of structure and function, the investigation is naturally divided for the greater part between the histologist and the physiolo-

gist. There is not a histological neuropathologist of course who would venture to say that our knowledge of the structure of the nervous system and the special higher division of the central nervous system has been brought to an end; on the contrary he will admit that the structure of these parts still remains in a state of high confusion and darkness. Has not the cytoarchitectonics of the cortex of the hemispheres, though readily scrutinized, been shown only recently to be extremely complex and diverse? And has not all this manifold variety in the organization of the different parts of the cortex been hitherto without definite dynamic meaning? If it is possible for the histologist to analyze the structure as yet only to a small extent, how can the physiologist expect to trace fully the action of the functional phenomena along this inconceivable network? The physiologist, keeping to the reflex scheme, never imagined an investigation of the central stations worked out in detail even to a limited extent in the simplest structure of these centers, but he constantly held to and was guided by the fundamental representation of the fact of transmission, the transfer of a dynamic process from the afferent to the efferent path. As regards the higher centers, besides the possibility of adapting function to the details of structure, he concentrates his attention and his work, for the present from sheer necessity, chiefly on the dynamics, on the general functional properties of the brain. This has been done and is being done till very recently, mainly by the schools of Sherrington, Verworn, and Magnus, and by other individual authors, on the lower levels of the central nervous system; but on the highest levels it is being done for the most part and in the most systematic way right now by me and my co-workers under the guidance of the conditioned reflex variety of the general reflex theory.

In regard to the cortex of the hemispheres, at the beginning of the notable epoch of the 70's of the last century the first indisputable data were obtained about the detailed connections between its function and its structure. Though the existence of special motor tracts in the cortex was confirmed and reconfirmed by all further investigators, the very exact

and limited localization of the organs of sense in the cortex, as originally described, soon met with objections on the part of physiologists as well as neurologists. This to some degree shook the doctrine of localization in the cortex. This uncertain state of affairs continued for a long time, owing to the fact that the physiologist did not have his own purely physiological characterization of the normal action of the cortex; and the treatment by psychological conceptions, at a time when psychology had not yet arrived at a natural and universally accepted systematization of its phenomena, was of course unable to assist in further experimentation on the question of localization. The situation changed radically when, thanks to the doctrine of conditioned reflexes, the physiologist at length received the means of viewing with his own eyes the special, though purely physiological, work of the hemispheres and thus was able to distinguish clearly the physiological action of the cortex from the action of adjacent subcortical parts and in general of the lower levels of the brain, in the form of conditioned and unconditioned reflexes. Then all the earlier facts, which had however been broken into, could be brought back into a distinct and strict order, and a fundamental principle of the structure of the hemispheres came out clearly. From the 70's on, the special tracts in the cortex which had been pointed out as centers for the chief external receptors remained the locations of higher synthesis and analysis of corresponding stimuli, but in addition to them representatives of these receptors were acknowledged to be scattered, perhaps throughout the whole cortex, certainly through the greater part of it, but available merely for the more simple and quite elemental syntheses and analyses. A dog without occipital lobes was unable to discriminate one object from another but did discriminate degrees of illumination and simplified forms. A dog without temporal lobes did not discriminate complex sounds such as his own name, etc., but did discriminate exactly separate sounds, for example one tone from another. What striking demonstrations of the fundamental significance of specialized structure!

Of interest as a more specific indication of the functional

significance of the structural characteristics of special tracts is the following experiment of Dr. M. I. Elliason, which was reported in my book, 'Conditioned Reflexes, an Investigation of the Physiological Activity of the Cerebral Cortex.' From three tones of a harmonium, two extreme and one intermediate, covering a range of over 3½ octaves, given simultaneously, a complex conditioned food stimulus was provided, which yielded a definite amount of saliva as an index of the intensity of the food reflex. When further tested, the component tones of the complex also separately produced salivary responses, but less than the whole complex, and intermediate tones between these also produced salivary responses, but to a still slighter degree. Then the anterior parts of the temporal lobes (Gyr. Sylviaticus et Ectosylvius) on both sides, with the anterior part of the Gyr. Compositus posterior were excised. The following occurred. When all conditioned reflexes to stimuli from the various analyzers were restored after the operation,—such as the conditioned reflex to the chord (this even before some others),—the reflexes to the component tones of the chord were tested anew. The high tone, as well as the intermediate tone next to it, lost its action. But the middle and the low tones with their intermediate tones retained theirs; the low tone even increased its action, which now became equal to the effect of the whole chord. But when the high tone alone began to be accompanied by food, then it quickly (from the fourth trial on) became again a conditioned food stimulus and acquired a significant effect, not a lesser but a greater one than before. From this experiment one can draw certain exact conclusions: in the first place, that in different centers of the special auditory field of the cortex are represented individual elements of the receptive auditory apparatus; second, that complex stimuli use only this area; and thirdly, that representatives of the same elements of the auditory apparatus scattered through a great part of the brain have no positive rôle at all in these complex stimuli.

When it is seen, as I saw it with conditioned reflexes in hand, that a dog with most of the posterior part of both hemi-

spheres removed orients himself with a high degree of exactness to skin and olfactory receptors, losing only complex visual and auditory relationships with his surroundings, *i.e.*, not differentiating complex visual and auditory stimuli; that a dog without the upper halves of both hemispheres, retaining fully a complex auditory relationship with his surroundings, loses only—with a striking isolation—the ability to orient himself with regard to hard objects met in the environment; and that, finally, a dog lacking almost all the anterior halves of both hemispheres seems to be completely incapacitated, *i.e.*, to be practically deprived of normal locomotion, of normal use of his skeletal movements,—but nevertheless by another indicator, namely, the salivary glands, there is evidence of complex nervous activity;—when all this is seen, is it possible not to be impressed with the paramount significance of the bare structure of the cerebral hemispheres in the fundamental problem of a proper orientation of the organism in its environment,—equilibrating with it? How then can we doubt the further significance of the more detailed features of the structure?

If one were to take the exact standpoint of our author, described further on in detail, he would have to bid the brain histologists throw away their work as unnecessary, useless. Who would not shrink from such a conclusion? Otherwise, all the details of structure which are revealed must sooner or later find their own functional significance. And therefore, along with further histological studies of cortical substance, carried on even more searchingly, it is necessary to pursue pure, rigorous physiological investigations of the activity of the hemispheres and of the adjacent parts of the brain, so that gradually one may be connected with the other, structure with function.

And this is what is accomplished by the theory of conditioned reflexes.

Long ago and firmly, physiology announced a constant connection of definite internal and external stimuli with the explicit activities of the organism in the form of reflexes. The theory of conditioned reflexes indisputably confirmed in

physiology the fact of temporary connection between stimuli of all kinds, and not merely the definite ones, external as well as internal, with definite units of activity of the organism, *i.e.*, along with the conduction of nervous processes to the higher centers, it also stated exactly the phenomena of their connection and disconnection. By this addition, of course, no essential change in the conception of a reflex has come to pass. The connection of a definite stimulus with a unit of activity of the organism remains, but without exception under conditions that are exactly defined. That is why this class of reflexes has been given by us the designation of 'conditioned,' to distinguish them from the reflexes which exist inborn; and these older ones are called 'unconditioned.' Thanks to this the investigation of conditioned reflexes rests on the same three principles of the reflex theory: the principles of determinism, of gradual and successive analysis and synthesis, and of structure. For us, the effect is constantly linked with the cause, the whole is further and further divided into parts and then synthesized anew, and function remains connected with structure in so far of course as that is permitted by the data of modern anatomical investigation. Thus there is opened up, so to speak, an unlimited possibility of studying the functions of the higher divisions of the brain, *i.e.*, of the cerebral hemispheres, and of the adjacent subcortex with the most complicated fundamental unconditioned reflexes of the latter.

We successively study the fundamental properties of the cortical substance, define the essential action of the hemispheres, and clear up the connections and interdependence of the hemispheres and the adjacent subcortex.

The fundamental processes of cortical activity are excitation and inhibition, their spread in the cortex in the form of irradiation and concentration, and their mutual induction. The special action of the hemispheres consists in an unceasing analysis and synthesis of stimuli entering (for the most part) from external surroundings as well as from within the organism, after which these impulses are directed to lower centers beginning with the adjacent subcortex and ending with the cells of the anterior horns of the spinal cord.

Thus all the action of the organism occurs under the influence of the cortex in a most exact and most delicate correlation or equilibration with the environment. On the other hand the adjacent subcortex sends a powerful stream of impulses from its centers to the cortex whereby the tonus of the latter is maintained. As the final result the center of gravity of the examination of the higher divisions of the brain is now being transferred to the investigation of the functional phenomena of the hemispheres and adjacent subcortex.

As stated above, the fundamental work of the cortex consists in analysis and synthesis of impulses flowing into the cortex. The variety and number of these stimuli are countless, even in an animal like a dog. The most suitable formulation for expressing this number and variety of stimuli would be to say that for individual stimuli there appear all gradations in the states both of individual cortical cells and of their various combinations. By means of the cortex it is possible to elaborate special stimuli from all stages and varieties of the process of excitation as well as of inhibition, in individual cells as well as in their various combinations. Stimulation from different intensities of one and the same stimulus, the relationship of stimuli, etc., may serve as an example of the first, and as an example of the second, different conditioned stimuli which produce hypnosis.

These countless states of the cells not only take form under the influence of a stimulus that is going on, not only exist at the time of action of the external stimulus, but remain also in the absence of these in the form of a system of different, fluctuating but more or less stable, degrees of excitation and inhibition. To give an illustration of this phenomenon. We apply daily for some time a series of positive conditioned stimuli of different intensity, and of negative stimuli, in one and the same sequence and with the same intervals between all of them,—and we obtain a system of corresponding effects. If, then, during an experiment we repeat after each interval only one of these positive stimuli, then it reproduces just the same fluctuations of the effect that all the successive stimuli together produced in the previous experiments; *i.e.*, the same

system present in the state of excitation and inhibition of the cortex will repeat itself.

Of course, it is not permissible to carry out at once any far-reaching correlation between dynamic phenomena and details of structure; but this correlation is by all means admissible, for the structure of the cortex is so variegated throughout its whole extent, and there is the fact, which we already know certainly, that only certain phases of synthesis and analysis of stimuli are admitted to one portion of the cortex and to none other. And this same point is decidedly confirmed by a further finding of ours. From a series of different auditory stimuli (a tone, noise of escaping air, beats of a metronome, bubbling, etc.), or from the mechanical stimulation of different parts of the skin, developed into conditioned stimuli, we can lead a single stimulus to produce an abnormal or pathological effect and in the meantime the others will remain quite normal. We arrive at this result not by a mechanical process but by a functional one,—by bringing a given point of stimulation into a difficult position, or by means of excessive strength of stimulation, or by a severe conflict at that point between the processes of excitation and inhibition. But how can this be interpreted except that excessive functioning, brought about by us on the part of a given minute detail of the structure, had brought it to destruction, as though rough treatment with a very fine instrument had spoiled or destroyed it? How fine and highly specialized these details must be, if other auditory and mechanical stimuli remain completely preserved and untouched! Such isolated destruction could hardly be produced at any time by mechanical or chemical means. After this, one cannot doubt that, if at present we sometimes do not see changes in the behavior of the animal after mechanical destruction of the cortex, this is only due to the fact, which is self-evident, that we have not yet analyzed the behavior of the animal in all its elements, and that the number of these elements must be tremendous. And therefore, a dropping out of some of them naturally escapes our attention.

I have let myself linger so long on our data for the purpose, in the first place, of making further use of them in the criticism

of the experiments and the conclusions drawn from them by Lashley, and, secondly, of showing once more how fruitful at the present time is the investigation of the cerebral hemispheres, based on the entire reflex theory with all its principles.

But what does Lashley bring up against the reflex theory? With what does he break it down?[4] First of all it is quite obvious that he conceives it in a peculiar way. Arbitrarily, not reckoning with physiology, he thinks of it all only in terms of structure, without a single word regarding its other principles. It is universally accepted that the notion of the reflex originated with Descartes. But what was known about the detailed structure of the central nervous system, especially in connection with its activity, in the time of Descartes? For the physiologico-anatomical distinction between sensory and motor nerves was not made until the beginning of the 19th century. It is evident that for Descartes the idea of determinism alone formed the essence of the notion of a reflex and from it issued Descartes' conception of the animal organism as a machine. In this sense all later physiologists interpreted the reflex, tying the individual actions of the organism up with the individual stimuli, at the same time gradually bringing to light the elements of nervous structure in the form of different afferent and efferent nerves and in the form of special paths and points (centers) of the central nervous system, until they finally gathered together the characteristic features of the functions of the latter system.

The chief actual grounds upon which Lashley's conclusion regarding the present harmfulness of the reflex theory is advocated and the new mode of conceiving the mechanism of the brain is recommended, are drawn by the author from his own experimental material. This material consists mainly of experiments on white rats which learn the shortest path to the food compartment in a more or less complicated maze. According to the author's experiments, it was shown that the

[4] Simultaneously with the above-mentioned paper read at the Congress of Psychology appeared the monograph entitled Brain mechanism and intelligence by K. S. Lashley, reporting more fully the author's own experimental material; I shall therefore, in my further statements, refer to the address and monograph without making any distinction between them, drawing on facts, conclusions, and discussions therein.

training is more difficult in almost the exact degree, the greater the destruction of the hemispheres in the first place; and aside from this, it is quite immaterial which parts of the hemispheres undergo destruction, *i.e.*, the result is determined solely by the mass of the hemispheres remaining intact. After some additional experiments, the author comes to the conclusion that "specific cortical areas, and association or projection tracts, seem unessential to the performance of such functions, which rather depend upon the total mass of normal tissue." Thus there is asserted to exist an original, but really quite inconceivable situation, that the more complex activities of the apparatus are performed without the mediation of the special parts and chief connections,—in other words, that the whole apparatus works somehow independently of its constituent parts.

And so the main question is: Why does the solution of the maze-problem, which is performed regularly more slowly, depend only on the *extent* of destruction of the hemispheres, regardless of the relationship of the *location* of destruction? And here one regrets that the author did not keep in mind the reflex theory with its first principle of determinism. Had he done so, the first question which he would have had to raise regarding the method of his experiments, would have been the following: By what means can the general maze-problem be solved by a rat? It surely cannot be solved without some directing stimulus, without some kind of cue. For if we accept the opposite view, notwithstanding its difficulties, then we would certainly have to show that the task can be actually carried out without any stimuli at all, *i.e.*, it would have been necessary to destroy all the rat's receptors at once. But who has done this and how can it be done? But if, as is natural to suppose, for the solution of the problem signals,—certain stimuli—are essential, then destruction of individual receptors or of some of their combinations is obviously insufficient. Perhaps all or almost all the receptors serve in the response, with a substitution of one for another separately or in some combinations. And for the rat, under the recognized conditions of its life, this is certainly the case. It is not difficult to

picture to oneself that in solving the maze-problem the rat can make use of olfactory, and auditory, and visual, and tactile, and kinesthetic stimuli. And since the special centers of these receptors are situated in different places in the hemispheres, and representatives of their single elements very likely exist scattered throughout the whole mass of the hemispheres, there always remains the possibility of their solving the problem, however much of the mass of the hemispheres we have removed,—and the solution is naturally all the more difficult, the less there remains of unimpaired cortical tissue. But if one assume that in the case under consideration the rat uses only a single receptor, or only a few of them, then it is first necessary to demonstrate this by special experiments which leave no doubt, *i.e.*, by letting each sort act separately, or in some combinations, and excluding the others. But no such experiments have been made, either by the author or anyone else, so far as I know.

It appears indeed strange that the author pays no attention whatsoever to all these possibilities and does not put to himself the question: What then appears to be the basis for the rat's action in overcoming the mechanical obstacles; what stimuli, what cues serve for the corresponding movements? He limits himself merely to experiments which involve the destruction of individual receptors separately and in certain combinations, which do not wipe out the habit, and he ends his analysis of the fact of the habit with the statement: "The available evidence seems to justify the conclusion that the most important features of the maze habit are a generalization of direction from the specific turns of the maze and the development of some central organization by which the sense of general direction can be maintained in spite of great variations of posture and of specific direction in running." Indeed —one can say—some kind of bodiless reaction.

As additional experiments by the author relating to the reactions in the maze, various incisions into, under, and across, were made both in the hemispheres and in the spinal cord, for the purpose of excluding altogether the association and projection tracts in the hemispheres and the paths leading to the

cord. But we must point out that all these, as physiologists well know, are only rough, approximate methods, and in no way decisive,—the more so, the more complicated the structure. This is much more true in respect to the gross and simple peripheral nervous system. Physiologists well know how difficult it is to completely isolate organs from the nervous connections with the whole body, and often only total excision of an organ from the body gives absolute assurance in this respect. Physiologists are quite familiar with the various crossings and loops, etc., in the peripheral nervous system. Let us recall for example the case of the antidromic sensory fibers (sensation) in the spinal roots and the innervation of a single muscle by fibers from different roots. Then how many times more diverse and more delicate must this, so to speak, mechanical immunity be in the central nervous system under the tremendous elaboration of its existing connections. It seems to me that up to the present, particularly in the physiology of the nervous system, this highly important principle has received insufficient recognition and has not even been formulated clearly and constantly. For the system of the organism developed itself in the midst of all its surrounding conditions: thermal, electrical, bacterial, etc., including also mechanical conditions; and it had to bring all these into equilibrium, to become adapted to them, possibly to anticipate or restrict their action when destructive to itself. In the nervous system and especially in its most complex central part, which rules the whole organism, and unites all the special activities of the organism, this principle of mechanical self-defense, the principle of mechanical immunity, had to arrive at absolute perfection, which it has actually done in the majority of cases. As we are unable at present to claim complete knowledge of all the connections in the central nervous system, all our experiments with incisions, sections, etc., practically appear in many cases to be merely negative, *i.e.*, we do not accomplish the final aim of severing them because the mechanism appears more complicated, so to speak more highly self-regulatory, than we had pictured it. And therefore to draw a decisive and far-reaching conclusion on the basis of such experiments is always risky.

In connection with our first question, I shall turn to the problem of the comparative complexity of habits, which the author was investigating. I shall do this chiefly for the sake of evaluating the methods which he uses. The author finds that the maze habit is more complicated than the habit of discriminating different intensities of illumination. But how is this demonstrated? In fact it is shown that, on the contrary, a habit in the most difficult of the mazes was formed in 19 trials, while the brightness habit was formed in 135 experiments, *i.e.*, seven times less easily. If a comparison be made with the simplest of the 3 mazes used by the author, then the difference in difficulty amounts to about 30 times. In spite of this the author comes to the conclusion that the maze habit is more complex. This is accomplished by means of various explanations, but, in order to carry conviction, he would somehow have to determine exactly, quantitatively, the significance of the several factors suggested in his explanation, showing why all of them taken together not only obscure the actual difference but even transform the result into its opposite.

With such a state of affairs, I would not venture to say what is complex and what simple. Let us come to the point. For the movements of the animal in the maze and in the box with different illumination we take into account only the turning to right or left, not every act of locomotion. In both problems cues or special stimuli are requisite for the turns. They exist in one and the other case. But beyond this a difference appears. In the maze there are several turns, in the box one. Therefore in this respect the maze is more difficult. But there is still another difference. In the maze the signs for turning are distinguished almost exclusively by their *quality*. For example, contact with the openings of the partition during turning occurs now on the right side, now on the left side of the body; in making the turns the muscles work alternately on the right and on the left. And this applies also to visual and auditory signals. In the box it is a matter of *quantitative* difference. These differences must somehow be evaluated. And, of course, the life habits of the rat must interfere, *i.e.*, the more or less early familiarity with one or

another problem,—as the author rightly points out. But it is also impossible to disregard the fact that in a very complex maze the problem is greatly facilitated by definite rhythm, by a regular alternation of turns now to right, now to left. On the other hand, in the habit of discriminating intensities of illumination we must take into serious consideration the fact that the formation of this habit arises under the influence of two impulses: food and nocuous stimuli (pain), whereas in the maze only food fixes the habit. And this of course complicates the conditions of training. Still another question: Do two impulses favor or impede the formation of a habit? Moreover, we pointed out above that the formation of a system of effects is a very easy and persistent thing in nervous activity. Thus in both methods, in the maze and in the box, we have as data different existing conditions, so that an exact comparison of the difficulties of the problem becomes almost impossible. All this, together with the uncertainty of the cues in the maze, as we noted above, make the entire method of the author to a considerable extent problematical.

That our author is more inclined to theorize—to draw conclusions—than to make refinements in his own various experiments (which is a fundamental requirement in biological experimentation), can be seen from the following two investigations of his, in connection with these experiments.[5] In one of these papers he investigates the visual habit formed to a given intensity of illumination. Having destroyed in a rat the occipital third of the hemispheres, he finds that the formation of the visual habit does not lessen the speed in comparison with the normal animal. But if that habit be formed in normal animals and thereafter the visual part of the hemispheres be removed, then the habit drops out and has to be formed anew. From this he draws the rather daring conclusion, which is sufficiently hard to conceive, that the process of training in general is independent of the site of injury, while the mnemonic trace or engram has definite localization. But

[5] K. S. Lashley, The relation between cerebral mass, learning, and retention, *J. Comp. Neurol.*, 1926, 41, No. 1. The retention of motor habits after destruction of the so-called motor areas in primates, *Arch. Neur. & Psychiat.*, 1924, 12, 249–276.

the matter is far more simple. In the occipital lobes, as we know, lies the special visual area, to which first of all come the stimuli from the eyes and where they enter into functional connections with one another for the formation of complex visual excitations, and also immediately into conditioned connections with the various activities of the body.

But since the visual fibers extend much further than just within the occipital lobes, probably throughout the whole mass of the hemispheres, then outside these special lobes they serve for the formation of conditioned connections with the various activities of the body, in the form of more or less elementary visual stimuli only. And if Lashley should form a habit not to the intensity of light but to an individual object, then the habit would disappear after removal of the occipital lobes and would not be formed anew. And thus the difference between the place of formation of the habit and the place of the mnemonic path would not appear.

In another research Lashley makes experiments on the motor tract in the cortex in monkeys. The motor habit does not disappear after removal of that tract. From this he draws the conclusion that the tract has no relation to that special habit. But in the first place, in his three experiments, he does not remove the tract entirely; perhaps the parts that remain are still sufficient for a mechanical habit of this given complexity. He sets this probability aside, not by experiment but only by argument. And again, besides the highly specialized motor area determined by electrical stimulation, there is perhaps a less specialized and more diffuse area. Accordingly, on these two grounds a more drastic complication of the mechanical problems is necessary. Finally, why has not the author blinded his animals?—for there is no doubt that in the manifestation of the habit vision has played a rôle, and stimulation in the lower motor apparatus might be effective through the visual cortical fibers as well. We meet striking examples of this in ataxic patients in cases of degeneration of the cord (tabes dorsalis). The ataxic subject can stand on one leg with his eyes open, but falls if the eyes are closed. Consequently in the first situation he replaces kinesthetic paths by visual.

Again a hiatus in the necessary further experimentation under the influence of his favorite negative attitude toward specific localization.

Let us turn now to other experiments and arguments of the author, aimed directly against the reflex theory. As regards the analysis of different adequate stimuli, the author says that there is surely no restriction to certain specific receptor cells which invariably take part in the formation of a habit and its reproduction, and that this is most evident in pattern vision. But in the first place we must see objects, *i.e.*, we receive definite combined visual stimuli with the help of every part of the retina, but not from the entire retina en bloc. And the effect is carried to the projection of the retina in the cortex. This is the reason why there is no definite connection between given receptor cells and a definite reaction. Only when we study an object in detail do we make temporary use of the fovea centralis; usually every part of the retina serves for a similar reaction to the given object. This principle applies also to the projection of the retina in the cortex. Second, as regards identity of response in the case of a geometrical white form on a black background and with the brightness relations reversed, with replacing of geometrical bodies by the corresponding contour outlines, and even with partial outlines,—on the one hand, what has just been mentioned covers this also, and on the other hand, this situation was long ago studied thoroughly, and it means that at first only the most general features of the stimuli act and only later, gradually, under the influence of the special conditions, a further analysis takes place and the more special components of the stimuli begin to act. In the given case, at first only combinations of white and black points without exact mutual relations and spacial distribution act as stimuli. This can be shown by the fact that with further special experiments a white figure on a black background can be differentiated with certainty from a black figure on a white background, *i.e.*, the mutual relationship of black and white will appear as a special stimulus. The same is true also in the replacing of a geometrical figure by a contour outline, etc. All these are but

gradual stages of analysis, *i.e.*, only step by step do the more detailed elements of the stimulus become stimuli in themselves.

In the group of reactions, *i.e.*, in the motor mechanism, the author points out that the rat proceeds correctly in the maze notwithstanding that it sometimes runs quickly, or again moves slowly, or even makes circus movements, as in case of injury to the cerebellum. And this appears to him an objection against a definite connection of the stimulus with a definite response. However, the rat moves constantly forward and makes turns now to the left, now to the right, with invariably the same muscles in the cases just indicated, and everything else is an additional movement, conditioned by other additional stimuli. Furthermore in the case of the exclusion of muscles throughout the formation of habits during paralysis and their subsequent use on cure of the paralysis, it is necessary to know where the paralysis lies and why it arose. For we have a huge series of coördinating centers, extending from the end of the spinal cord up to the hemispheres, and impulses from the hemispheres may go to one and all. Further we know that with every thought of movement we actually produce it implicitly. And so a process of innervation can occur, although it does not take visible form. And again, if the stimulation cannot be made effective through the nearest path, by the principles of summation and irradiation it has to pass to the most available centers. Have we not long known of the case where a decapitated frog, in wiping off acid placed on the thigh of one side with the foot of that side, if it is unable to do so because of the amputation of that foot, after a few unsuccessful attempts with the disabled extremity, makes use of the foot on the other side?

The allusion to the absence of stereotyped movements in some forms of activity, for example in the building of nests by birds, is also based on a misapprehension. Individual adaptation exists throughout the whole extent of the animal world. And this, precisely, is the conditioned reflex, the conditioned reaction, which takes shape according to the principle of simultaneous action. Finally, his allusion to the monotony

of grammatical forms agrees entirely with our previously adduced fact of the working out of systems in the nervous processes in the hemispheres in action. This is the combination or fusion of structure with function. Granted that we cannot now picture clearly how this comes about,—this is surely only because we do not yet know thoroughly either the structure or the mechanism of the dynamic processes.

I deem it superfluous to dwell further on the author's arguments against the significance of structure in the central nervous system. The features common to all this are the result of his failure to take into consideration at all the complexity of this structure, already known, with its further possibilities; whereas in a prejudiced way he constantly simplifies it to the bare scheme of a physiological textbook, which aims merely to point out an indispensable connection between stimulation and effect—and nothing more.

What then does our author offer in place of the reflex theory which he rejects? Nothing except more remote and altogether unjustified analogies. In seeking a solution of the problem of the higher brain mechanism, can one point to the tissue of fungi or hydroids, or to embryonal tissue, when in the higher part of the brain of higher animals including man we have the acme of differentiation of living matter? In any case, recognizing absolute freedom of hypothesis, we have the right to demand of the author at least a preliminary and elementary program of definite problems for immediate and fruitful experimentation upon this subject,—a program which can be profitably compared with the reflex theory, a program which would necessarily bring about an energetic advance in the problem of cerebral functions. But the author has practically no such program. A real and useful scientific theory must not only embody all existing material, but must also open up a wide possibility of further study and, one can say, of unlimited experimentation.

And such is at present the position of the reflex theory. Who will deny the extreme complexity,—scarcely imaginable by anyone,—of the structure of the central nervous system in its highest types, in the form of the brain of man, and the

necessity for a more profound study of it by improved methods? On the other hand, on this very account, the human mind remains overwhelmed by the riddle of its own activity. The reflex theory strives to provide unmistakably a possible ground for both one and the other, and thus to interpret the striking rôle, so difficult to conceive, of this most extraordinary mechanism. The possibility of experimentation on the brain, and especially its higher parts, by means of the reflex theory with its requirements of constant determinism and unremitting analysis and synthesis of the underlying phenomena, is actually without limit. This I have felt and have seen throughout the past thirty years without intermission; and the further I have gone, the greater my conviction.

III

Now that I appear in the psychological literature for the first time, it seems to me a fitting opportunity on the one hand to consider a few tendencies of psychology, which in my opinion do not accord with the aims of successful investigation, and on the other, to emphasize more sharply my viewpoint on this our common field of work.

I am an empirical psychologist and I know psychological literature only through a few leading psychological texts and, compared with the available material, through an altogether inadequate number of psychological articles which I have read. But from the time when I became really conscious of life, I have been and am still a constant observer and analyzer of myself and others in the range of life that is accessible to me, counting in also the best literature and genre painting. I reject point blank and have a strong dislike for any theory which claims a complete inclusion of all that makes up our subjective world, but I am unable to give up analyzing it or interpreting it simply, in its individual points. And this interpretation must result in bringing into accord these individual phenomena and the data of our modern positive knowledge in the natural sciences. And for this it is necessary to endeavor constantly to apply these data to every individual phenomenon in the most elaborate way. And I

am now convinced of this: that a purely physiological interpretation of much of what was formerly termed psychic activity has reached firm ground, and with the analysis of the behavior of the higher animals up to and including man, it has the right to make every effort to interpret the phenomena in a purely physiological way, on the basis of established physiological processes. In the meantime it is clear to me that many psychologists jealously, so to speak, guard the behavior of animals and man from such physiological explanations, constantly ignoring them and not attempting to apply any of them to any extent.

In confirmation of the statements just made, I take two very simple cases: one mine and the other Professor Köhler's. One could present many others, some much more complex.

When we were working out a method of feeding an animal from a distance at the time of the experiment, we tried out many different methods. This among others: In front of the dog there was always an empty pan, to which a metal tube led down from a container above, which held the dried meat-powder and usually served to provide the food for our animals at the time of the experiment. At the junction of the container and the tube was a valve, which was opened at the proper moment by means of air transmission, so that a portion of the powder dropped down the tube and came out into the pan where it was eaten by the animal. The valve was not in good working order and if the pipe were shaken some of the powder from the container would drop into the pan. The dog quickly learned to make use of this, of his own accord shaking out the powder. And a shaking of the pipe took place almost continuously, when the dog was eating the portion of food which was given it and in doing so knocked up against the pipe. Of course this is exactly what takes place in training a dog to give one his paw. In our laboratory work, the conditions of life have in general done the teaching, but here, man forms part of the conditions. In the latter case the word 'paw,' 'give,' etc., the skin stimulation from the contact in lifting the paw, the kinesthetic stimulation accompanying the lifting of the paw, and finally the visual

stimulation from the trainer, were accompanied by food, *i.e.*, were bound to the unconditioned stimulus for food. It is absolutely the same in the instance cited: the noise of the shaking pipe, the skin stimulation from contact with the pipe, kinesthetic stimulation in jostling against the pipe, and finally the sight of the pipe—all these became similarly connected with the act of eating, with excitation of the feeding center. This of course occurred through the principle of simultaneous association, presenting thereby a conditioned reflex. And here, moreover, two additional distinctly physiological facts appear: In the first place, the definite kinesthetic stimulation in this case is probably linked up by a conditioned setting (in the lower parts of the central nervous system by an unconditioned setting) with the execution of those movements or the activity which produced it,—this kinesthetic stimulation. And second, when two centers in the nervous system are connected or joined, nervous impulses are set in motion and pass from one to the other in both directions. If we accept the absolute law of one-way conduction of nervous impulses in all points of the nervous system, then in the case cited one must assume an additional connection in the opposite direction between these centers, *i.e.*, one must grant the existence of an additional neurone connecting them. When food is given on raising the paw, a stimulus undoubtedly runs from the kinesthetic center to the feeding center. But when the connection is established, and the dog, under the urge for food, gives his paw himself, obviously stimulation runs in the opposite direction. I can interpret this fact in no other way. Why this is merely simple association, as psychologists usually assume, and by no means not an act of intelligence, of ingenuity,—even if of elementary things,—remains unclear to me.

The other example I take from W. Köhler's book, 'Intelligenzprüfungen an Menschenaffen,' also referring to dogs. A dog is placed in a large cage situated in an open space. Two opposite walls of the cage are solid, through which nothing is seen. Of the other two walls one is a screen, through which clear open space can be seen; the other (opposite) has

an open door. The dog stands in the cage behind the screen, and *at some distance* in front of the screen a piece of meat is placed. As soon as the dog sees it, he turns around and goes through the door, around the cage and takes the meat. But if the meat lies *close* in front of the screen, then the dog vainly pushes up against the screen, trying to get the meat through the screen, and does not use the door. What does this mean? Köhler does not attempt to decide that question. With conditioned reflexes at our disposal we understand the matter easily. Meat lying near at hand strongly stimulates the olfactory center of the dog and that center, by the principle of negative induction, strongly inhibits the rest of the analyzers, the other parts of the hemispheres, and thus the track to the door and the roundabout way remain under inhibition, *i.e.*, subjectively expressed, the dog has temporarily forgotten them. In the first case, in the absence of a strong olfactory stimulation, this trace remains under little or no inhibition and leads the dog more correctly to its goal. At all events, this explanation underlies the matter and harmonizes with further exact experimental proof. In confirmation, his experiment would reproduce the mechanism of revery, of strong concentrated thinking about something, when we do not see or hear what is going on around us, or, somewhat similarly, it reproduces the mechanism of what is termed blindness under the influence of passion.

I am certain that with persistent experimentation many other and more complicated instances in the behavior of animals and man would also prove interpretable from the point of view of many established rules of higher nervous activity.

The second point which I shall take up relates to the question of the significance of the aim and purpose of psychological investigation. It seems to me that on this point there invariably arises a confusion of different things. Before us is the sublime fact of the evolution of nature from the primordial state in the form of nebulae in space, all the way up to human beings on our planet,—in the form, to put it roughly, of phases: of the solar and planetary systems, of the inanimate

and animate part of nature on the earth. In living matter we see especially strikingly the phases of evolution in the form of phylogeny and ontogeny. We still do not know, and probably will not know for a long time to come, either the general law of evolution, or all its successive phases. But seeing its manifestations, we anthropomorphically, subjectively, both in general, and in particular, replace the knowledge of the law with the words 'aim,' 'purpose,' *i.e.*, we merely repeat the fact, adding nothing to our present knowledge of it. But in seeking out the truth concerning the separate systems of which nature consists, up to and including man, it all boils down to a mere statement of the internal as well as the external conditions of existence for these systems,—in other words, to the study of their mechanism; and thrusting into this experimentation the idea of purpose in general simply results in a medley of different things and becomes a hindrance to lines of investigation that are accessible to us and that are immediately fruitful. The idea of a possible goal in each system can serve only as a help to our study or the use of scientific imagination for the sake of suggesting new questions and a variety of experiments,—just as when we seek to gain familiarity with a machine of which we are still ignorant, and which is the work of human hands;—but it is not the final aim.

With this is naturally connected the next question,—the question of freedom of the will.

This question is of course one of the greatest practical importance. But it seems to me there is a possibility of discussing it, both scientifically (on the basis of contemporary exact natural science), and at the same time not inconsistently with the feeling regarding it that is common to all men, and without involving confusion in its essential formulation.

Man is of course a system,—roughly speaking, a machine, —like every other system in nature subject to the unescapable and uniform laws of all nature; but the human system, in the horizon of our contemporary scientific view, is unique in being most highly self-regulatory. Among the products of man's hands, we are already familiar with machines which regulate themselves in various ways. From this standpoint the method

of investigating the system of man is precisely the same as that of any other system; decomposition into parts, study of the significance of each part, study of the connections of the parts, study of the relations with the environment, and finally the interpretation on this basis of its general workings and administration, if this be within the capacity of man. But our system is self-regulatory in the highest degree,—self-maintaining, repairing, readjusting, and even improving. The chief, strongest, and ever-present impression received from the study of the higher nervous activity by our method, is the extreme plasticity of this activity, its immense possibilities: nothing remains stationary, unyielding; and everything could always be attained, all could be changed for the better, were only the appropriate conditions realized.

The system or machine—and man with his host of ideals, aspirations, and achievements—what a terrifying, discordant juxtaposition this seems at first glance. But is it really so? For according to the view of evolution, is not man at the summit of nature, the supreme personification of the resources of a nature which is without limit, the realization of its powerful but still unknown laws? Is not this sufficient to maintain the dignity of man, to fill him with highest satisfaction? And there still remains in life all that is also embraced in the idea of freedom of will with its personal, social, and civic responsibility; for me there remains this possibility, and hence also the obligation for me to know myself, and constantly, using this information, to maintain myself at the utmost height of my capabilities. Are not the social and civic duties and requirements, situations which present themselves to my system, and which must lead to appropriate reactions that will promote the integrity and perfection of the system? [6]

[6] I am greatly indebted to Dr. R. S. Lyman, who kindly assumed the difficult task of this translation; he has executed it with special care, both in regard to the subject matter of the article and to the particular style of the Russian text.

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THE ORGANISMIC HYPOTHESIS AND DIFFERENTIATION OF BEHAVIOR

I. THE CELL THEORY AND THE NEURONE DOCTRINE

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During the course of some investigations on the behavior of newborn infants (**16**) it became very obvious that the traditional theories on infant behavior were inadequate to account for the nature of specific reactions in infants. The reflex chain theory of the physiologists did not seem to fit the development of specificity of activity. Consequently a survey of the literature, particularly in the field of experimental biology, was made, directed specifically toward the biological phenomena included under what is known as the organismic theory. This view attempts to avoid certain atomistic implications of the reflex theory. The more significant studies found in the survey are included here on the assumption that a presentation of the criticisms of the atomistic interpretation and a statement of the organismic point of view, as it has been formulated by zoölogists, and of its limitations, should prove of theoretical value in infant research as well as in the broader field of psychology. Furthermore, an attempt is made to show that atomism is exemplified in biology by the cell theory, the neurone doctrine, and the reflex arc concept.

The organismic hypothesis was clearly stated by de Bary (**3**) about the middle of the last century. In England, Sedgwick (**37**) urged the inadequacy of the cell theory. In America the organismic view took its rise at the beginning of the nineties in the field of zoölogy as a reaction against a too rigidly atomistic conception of the cell. Wilson (**47**) from his studies on cleavage in the egg of amphioxus held that varia-

bility appearing in the blastular stages argued for a revision of the Roux-Wiesman theory of development. A critique which went farther than that of Wilson's was made by Whitman (**46**) in a paper on 'The Inadequacy of the Cell Theory of Development,' read before the Zoölogical Congress of the World's Columbian Exposition. The same position had been stated by him five years earlier. Since Whitman the subject has been controversial in biology, especially in cytology, although Sharp (**38**) in the new edition of his 'Introduction to Cytology' has come out definitely for an organismic interpretation of the cell. A good statement of this view as stated by Sharp is: "According to this general interpretation, ontogenesis is a function primarily of the organism as a whole, and consists in the growth and progressive internal differentiation of a single protoplasmic individual, this differentiation often, but not always, involving the separation of the living mass into subordinate semi-independent parts, the cells. Since the septation is rarely complete, all parts remain in connection and the whole continues to act as a unit. Thus development is not primarily the establishment of an association of multiplying elementary units to form a new whole, but rather the resolution of one whole into newly formed parts: it should be thought of not as a multiplication and coöperation of cells, but rather as *a differentiation of protoplasm*."

The influence of the organismic movement was not distinctly felt in psychology until more than a quarter of a century had elapsed after its introduction into American zoölogy by Whitman (**46**). During the second decade of the present century its basic idea appeared in German psychology as an aspect of Gestalt. American psychology was much more influenced by Gestalt in the direction of organismic thinking than by the results in experimental zoölogy.[1] Nevertheless in the present decade the organismic view has been given decided impetus by the publication of the long and careful researches of Child (**6**) on physiological gradients, and of

[1] The term zoölogy in this paper is used in its broader meaning. It includes cytology, histology, morphology, physiology, neurology, embryology, and animal reactions. The psychology of the human organism is assumed to be primarily a zoological study comparable with these groups.

Coghill's studies (7) in neural anatomy and behavior patterns in Amblystoma. The effect of these purely zoölogical investigations perhaps have influenced psychology indirectly, but an assault on the reflex theory which more directly and profoundly influenced the thinking of psychologists is the work of Lashley, reported in a series of papers during the last dozen years. Other related studies in zoölogy are those by Detwiler (9), Hyman (14), Langworthy (21, 22), Nicholas (32), Swenson (40, 41), Tracy (42), and P. Weiss (44); in animal behavior and psychology, Avery (2), Cameron (5), and Richter (35); and the attempt to trace the influence of the organismic view in the natural sciences and in philosophy by Ritter and Bailey (36). An especially valuable study in the field of animal psychology is that by Lashley and Ball (24). In systematic psychology certain aspects of the organismic view have been treated by Kantor (17), Meyer (31), and Weiss (43). Ogden (33) has undertaken the task of interpreting Gestalt principles on the educational side, and Wheeler's recent book (45) is the latest treatment of these principles. The most authoritative recent expression of Gestalt is by Köhler (20). The foregoing are merely a few representative researches, but the net result of them is to indicate definitely that the theory which holds that an organism or its behavior is a composite of independent elementary units has been losing its status in zoölogy, in physiology, and in psychology.

An object of the present series of papers is to point out that the neurone theory and the reflex arc concept are logical corollaries of the cell doctrine. With the cell doctrine dominant in biological science during the last century and largely so in the present one, it is no accident that the neurone should have come to be considered the structural unit, and the reflex arc the functional unit of the nervous system. These three conceptions involve essentially the same atomistic type of analysis. A consideration of them will perhaps make this apparent.

The Cell Theory

The cell theory was developed by biologists about a hundred years ago and at once became a leading biological concept whereby the science of biology rapidly advanced to its present status. It is probable that no other concept in this field has so profoundly influenced the development of biology. The cell theory in biology is comparable to the atomic theory in chemistry. It assumes very definite units of structure which are the elements of the organization of the organism. de Bary (3) was the first important biologist to challenge the limitations of the theory.

Whitman (46), who was the earliest among American zoölogists to subscribe to the newer interpretation, opposed the cell theory on the grounds that development of organization in a multicellular form is fundamentally no different than that of a single cell form. The essential difference does not lie in the number of cells. "How difficult it is," he says, "for example, to regard a multicellular nephridial funnel as the exact homologue of the unicellular funnel. If the organ consists of one cell, the tube is *intra-cellular;* if of many cells then it is *inter-cellular.* But we have the tube just as perfect with one cell as with many. How idle, then, to deny homology between two such organs merely because one is intra- and the other inter-cellular. And yet this is precisely what we have been accustomed to do.

"Now this one case illustrates, as I believe, a general truth of no little importance. The nephrastome is a nephrastome all the same whether it consists of one cell, two cells, or many cells. Its form and function are both independent of the number of component cells. Cells multiply, but the organ remains the same throughout. As the nephrastome is not the result of cell-formation, but exists before division into cells, so the organism exists before cleavage sets in, and persists throughout every stage of cell-multiplication" (46).

Whitman summarizes the case in the following language: "Comparative embryology reminds us at every turn that the organism dominates cell-formation, using for the same purpose one, several, or many cells, massing its material and

directing its movements, and shaping its organs, as if cells did not exist, or as if they existed only in complete subordination to its will, if I may so speak" (46). Attention, perhaps, should be called to the fact that this was written more than twenty years before the Gestalt school emphasized the dynamic aspects of the organism.

In the same year that Whitman was championing the cause of the organism as a whole in zoölogy, Wilson (47) published the results of his study on cleavage stages in amphioxus. He found that cleavage of the ovum of amphioxus exhibits what he characterized as a 'protean variability.' By means of mechanical disturbances at various stages of cell cleavage he was able to demonstrate that development in this form is capable of a very high degree of artificial modification. The conclusion which he draws from these observations is this: "The experiments on the alteration of cleavage forms by pressure, temperature, and other agents, taken in connection with the phenomena exhibited by isolated, or partially separated blastomeres, show that the embryo develops as a whole, as a unit, and demonstrates the principle urged by Whitman that the organism, as a whole, controls the formative processes going on in each part."

Although Wilson espoused an organismic view, it should be stated that he attempted to reconcile it with the mosaic view, and consequently his position is not as thoroughgoing as that of Whitman.

The extensive observations and experiments of Lillie on the embryonic development of the annelid Chaetopterus are significant. A single instance will be representative of these experiments. "On July 28, 1904, a batch of eggs from one female was divided into three parts, which were fertilized after standing three hours, four hours, and five hours respectively in sea-water. None of these segmented to any considerable extent. The next morning the four hour lot was swimming with ciliated specimens—all of which had developed without cleavage," (27, p. 221). After considering the possibility of an elementaristic mode of explanation, Lillie continues: "My own views agree most nearly with those ex-

pressed by Whitman in his paper on 'The Inadequacy of the Cellular Theory of Development.' If any radical conclusion from the immense amount of investigation of the elementary phenomena of development be justified, this is: that the cells are subordinate to the organism, which produces them, and makes them large or small, of a slow or rapid rate of division, causes them to divide, now in this direction, now in that, and in all respects so disposes them that the latent being comes to full expression. I believe that this conclusion is strongly reinforced by my observations on differentiation without cleavage; for here we see the various substances of the ovum marshalled in order, disposed in a bilateral arrangement and fashioned in the form of a larva: and we seen that cilia and other cell-constituents arise in the appropriate relations—and all this without the need of a single nuclear division" (**27**, p. 252).

Thus it is seen in those branches of zoölogical sciences chiefly concerned with the cell that there is a definite tendency away from an atomistic view toward an organismic standpoint. This tendency has been so effectively described by the Dutch histologist Boeke (**4**) that we can do no better than to use his own words. "It cannot be denied that animal histology nowadays is in a period of unrest, of evolution, of reform; that it is passing through a crisis which is shaking its very foundations. The old conception of the cell as an elementary organism, established as the basis of all our researches after years of strife and an incredible amount of labor—this conception of the cell, on which generations of our teachers had built up the entire splendid structure of our knowledge of the animal body, must be revised. Obviously the cytologist, when studying and analyzing the structure of the smallest element, is tempted to regard these cells as units. On the other hand, when by the use of accurate and subtle methods of present day histology, we penetrate farther and farther into that wonderful organization of the cells, the cell itself loses part of its independence as an elementary organism. Many of these we see in the course of development losing their boundaries and fusing together to form a syncytium. Every-

where new elements are being formed, not always entirely independent of each other, and out of a syncytial stage these elements appear as new cells. Every organism is a unity in itself, and the elements which compose it are in the first place a part of the whole, deriving their full value not from themselves, but from the individual to which they belong. The individual, the organism as a whole, dominates the cells which compose it" (4, p. 4).

The Neurone Doctrine

The objection to the atomistic conception of cells extends with equal validity to the neurone which is a specialized cell. In Lickley's little handbook on the nervous system occur these words: "The structural unit of the nervous system is a highly specialized cell consisting of a cell body and a series of processes; the complete unit is called a neurone. The functional unit consists of a pair of such neurones" (26, p. 6). Practically every textbook published since 1900 in zoölogy, neurology, and psychology has carried a comparable statement. This is, of course, atomism of a neural type, and is the logical corollary of the atomism involved in the cell theory. The objection to this interpretation of the neurone is based on histological observations, on experiments in nerve regeneration, and on the results of investigations in the field of electro-physiology.

The researches of Boeke (4) on the myoneural junction or the end-plate have raised problems which cannot be adequately explained on the view that the nerve cell is an independent entity. He has observed by staining methods that the motor end branches on striped muscle fibers of the hedgehog are not separate, independent structures, but that there is a continuity of tissue between the nervous fibrils and the muscle fiber. The neurolemma passes into the sarcolemma and the protoplasm of the axis cylinder is continuous with the contractile portions of the muscle. He has likewise observed continuity between the nerve ending and smooth muscle cells in the case of the ciliary body of the human eye. "Here too the terminal nerve fibrils pass into the cell, ending

within the protoplasm with loop-like expansions, which often are seen lying so close to the nucleus of the muscle cells, that they even make an indentation and thus are found lying in a shallow cavity in the side or on top of the elongated nucleus, thereby proving their intraprotoplasmic position" (4, p. 7). Thus even in such a minute structure as the neuro-muscular junction the atomistic principle is questionable. The neurone is part of the whole and not a separate thing in itself.

Boeke (4) has been able to follow the processes of regeneration in motor nerves after they have been severed. He has demonstrated that regeneration is not a process confined alone to the new outgrowing axon, but that it is a total process in which all the surrounding tissue-cells are seen to engage. None of these cells remain passive and none predominate in the process. Nowhere does a fibrillar strand run free in the interstices between cells. These observations have been carried out on tissues suitable to the study of regenerating end-ramifications such as the corpuscles of Meissner, the corpuscles of Grandry in the skin covering the duck's bill, corpuscles in the snout of the pig, the organ of Eimer in the mole, the muscle spindles of cross-striated muscles of different mammals, and human smooth muscle.

The regeneration of the end-plate shows two characteristic stages. The first appears when the out-growing fiber reaches the muscle. Its delicate end-ramifications are entirely different from normal motor-endings. They are more exuberant, complicated, and bizarre. Nevertheless, at this stage functional continuity between nerve and muscle is established. If the experimental animal is kept alive some months the second or normal stage appears. The regenerating motor nerve endings lose their exuberance of form and by degrees the normal form of the ending characteristic of the species studied eventuates. Boeke, therefore, suggests that regeneration is not alone a restoration of function, but is also a restoration of an equilibrium of form.

Thus both the outgrowth of the fiber and the reconstitution of the end-plate exhibit a harmony and coöperation of various kinds of tissue which an elementaristic conception

overlooks. All the surrounding tissues act together. The scar tissue which grows between the cut ends of the nerve is composed of delicate elongated branched cells, all fused to form a syncytium. Within this syncytium the delicate neurofibrillar strands are always found. They run intra-protoplasmically from one cell of the syncytium to the other. It is this syncytial aspect of the regenerating process which led Boeke to an organismic view of the neurone. He says, "And now, after having studied these things in the muscle spindle, when turning again to the ordinary muscle-fibers, we detect everywhere traces of the same process going on here, of the same conductive tissue forming between the muscle-fibers. And I can imagine no other process which affords such a striking example of the elements of the different tissues as subordinate parts to the whole, to reach a given end, the restoration of the equilibrium of the organism, than this mode of regeneration of the motor nerve endings" (4, p. 12).

In summarizing the data of his researches in their bearing on the organismic interpretation of the neurone he concludes, ". . . among the different parts of the organism it is the nervous system which, if this conception be true, must in the first place exhibit phenomena in harmony with this view. And we do not look in vain. The neurone theory—the nerve cell a distinct unity, even embryologically, histologically, functionally, a separate independent organism, a true cell of the old school, has to be revised" (4, p. 5).

In this connection attention may be called to the phenomena to which Kappers (18) has given the name 'neurobiotaxis.' Kappers' theory in a modified form has been adopted by Child (6) and has been given experimental verification by Ingvar (15) and by Lund (30).

Kappers (18, 19) formulated a law of neurobiotaxis in the developing neurone: "When from different places stimuli proceed to a cell, its chief dendrite grows out and its cell-body shifts in the direction where the majority of stimuli proceed" (18, p. 264). An important question is, not only why dendrites and cells move toward the center of stimulation, but why the axone moves away from it in the direction of the

stimulus-irradiation. To explain this Kappers has recourse to the experiments on galvanotropic phenomena. It is known that if the root of the sprouting seed of the bean be exposed to a constant current, the tips of the root turn and grow toward the negative pole. Ameba, on the transmission of a constant current through the surrounding medium, sent out enlargements and finally shifted in the direction of the cathode. It was found that when a paramecium in pure water or in a weak solution of common salt, was influenced by a constant current, the movement was in the direction of the cathode, but that this direction of galvanotaxis may be reversed by placing the animal in a stronger solution of salt. If in the latter case the constant current were found through it, a movement towards the anode was observable.

"There is much evidence that these galvanotropic experiments are applicable to the formation of the nervous system by the stimuli that reach it and act in it. We know from the negative variation that a part of our nervous system which is stimulated forms a negative pole, a cathode, with respect to its surroundings, which in other words form an anodic field with regard to the center of stimulation. The nerve-cells which are found in the surroundings of this electronegative center of stimulation will first show an anodic offshoot going in the same direction as the radiation from that center of stimulation on account of the anodotropic character of their protoplasm. This anodic extension will derive chemical and tropic characteristics of the potassium and chlorides in which it is embedded. In consequence a larger quantity of potassium chloride is found in the axis cylinder than elsewhere in the neurone. This large quantity of chloride of potassium will again enhance the anodotropic, in casu stimulo concurrent character of the axon, and besides it increases conductivity. Not until much later do the dendrites appear and somewhat later still the cell-body begins to move in the direction of the electronegative center" (**18**, p. 296).

Child suggested that "the electrical factor is effective, not directly as electrical, but only through the changes in the physiological state brought about by it and the localization of those changes in relation to the electrical currents" (**6**, p. 182).

Lund (30) furnished experimental evidence for this view. He has investigated the control of polarity in Obelia by the electric current. He found that internodes cut from Obelia commissuralis possess and retain an inherent polarity, according to which regeneration of a hydranth occurs at the apical end before that at the basal end of the internode and the apical internodes develop hydranths earlier than internodes of basal origin. However, with appropriate density of electric current, all hydranth formation on ends of internodes when turned toward the cathode can be delayed or entirely inhibited, while at the same time hydranths may form in a high percentage of pieces on ends toward the anode. Thus the inherent and normal polarity of the internode may be reversed by means of the electric current. Lund concludes, therefore, that the development of an electrochemical polarity is probably a primary, fundamentally associated condition for the origin of morphological polarity, because the physiological conditions which determine morphological polarity can be controlled and directed by an external electric current.

Ingvar (15) is credited with performing a highly important experiment on the influence of the electric current on growth of neural tissue in the chick. He observed that the galvanic current has a directing influence on the cell and fiber outgrowth in cultures. These outgrowths proceed almost entirely along the lines of force in the galvanic field toward the anode and the cathode. On the other hand the outgrowths in control tissue proceed in all directions. The experimental tissue reveals that cell processes are morphologically different, depending on whether they grow toward the anode or toward the cathode. Moreover if a weak current by means of a single conductor is drawn through the culture, Ingvar (15) observed that the cell outgrowths proceeded perpendicular to the conductor, resembling the outgrowths from the spinal cord growing down the cord. His conclusion to these findings is that, "The hypothesis of Kappers, as the main result of this author's work on neurobiotaxis that electric forces are determining factors in the outgrowths and distribution of the different constituents of the nervous system, has been proved

to be a fact in pieces of central nervous system of the chick cultured in vitro. As several authors have pointed out, electrical currents flow in developing organisms. The currents successfully employed in our experiments correspond in electromotive force with those found in various embryos. From this it may be concluded that electrical forces play a role in the formative processes in morphogenesis " (**18**, p. 199). Kappers (**19**) himself concurs in Ingvar's conclusion and looks upon this experiment as definitely demonstrating his theory of neurobiotaxis.

Moreover, researches in electro-physiology indicate one method of how the organism and its tissues contribute to the response of the organism. The discovery that the energy of the nervous impulse is released locally in the neurone in an all-or-none manner has eventuated in a new body of facts concerning time relations during conduction. The exploitation of this approach is scarcely begun, but already the functions of all-or-none, chronaxie, refractory period, and frequency of impulse in nerve and muscle have rendered the descriptions of correlation, learning, and development in terms of 'bonds,' 'connections,' and 'reflexes' altogether too simple. The excellent researches of Adrian (**1**) , Erlanger and Gasser (**10**), Gotch (**11**), Lapicque (**23**), Lillie (**28**), Lucas (**29**), and Pratt and Eisenberger (34), on the conduction of the impulse promise to change completely the face of these problems. Thus in the all-or-none propagation of local disturbance is neatly illustrated the principle that the organism is not a mere inert mass played upon by external forces alone, but on the contrary itself makes an energy contribution which cannot be overlooked in any account of the nature of the response. And further in the effects of chronaxie and refractory period is revealed how the organism itself imposes temporal limitations on the behavior of its parts.

Thus, the assumption of the organismic view that the organism itself influences both its own developing structure as well as its function is illustrated on the one hand by the phenomena of neurobiotaxis, and on the other hand, by the energy and time relations of its tissues. Any protoplasmic result,

whether of development or of functioning, is participated in by the organism. Moreover, the work of the electro-physiologists on all-or-none, chronaxie and refractory period shows how emphasis in the study of the neurone has swerved from a purely morphological analysis of structures to an analysis of neural functions.

Transplantation Experiments

The experimental embryologists have also furnished a considerable body of evidence which shows that the elemental theory of development is inadequate and that development in one part of the embryo is in part dependent upon other factors within the organism.

The primordia from which the lens of the eye of the frog develops is a thickening of the under layer of cells of the ectoderm. It develops exactly opposite the end of the optic vesicle that pushes out from the midbrain. Lewis (**25**) demonstrated that the location of the lens is not an independent function, but is determined by the optic vesicle. He showed this to be true in two ways. He cut off the end of the vesicle and inserted it beneath the ectoderm of a nearby region of the head of the same or of another embryo. The ectoderm opposite the new location of the vesicle thickened and formed a lens. The second method was to excise the ectodermal area opposite the optic vesicle at a stage just previous to the formation of the lens. Then a piece of ectoderm from another part of the embryo, or from another embryo and even from an embryo of another species was grafted upon the site of the primordial area. The new ectoderm developed the lens.

Spemann's work (**39**) on the plasticity of tissues of embryo of Triton taeniatus and Triton cristatus are highly significant in this connection. The mass of cells lying anterior to the dorsal lip of the blastopore of these amphibian embryos has been designated by him as the 'organizer.' From this mass of cells there extends a differentiating influence whose nature is not thoroughly known. However, the fact of their dominating influence on the rest of the organism has been thoroughly demonstrated in many experiments. If the organizer is

extirpated and grafted again upon the body of the organism in a reversed position—that is, at 180°,—the structures develop accordingly. That is, the cells which formerly would have developed into skin tissue now may become nerve tissue. The cell that would have produced an eye will develop midbrain or ear. In other words, the fate of any tissue previous to gastrulation may be altered by altering the position of the organizer. The graft may be very exactly observed by transplanting the light tissue of one of the above forms upon the darker tissue of the other.

Extensive experiments on limb transplantation in Amblystoma have been conducted and reported by Harrison (**12**) and Detwiler (**9**) in a long series of papers. In Germany, P. Weiss (**44**) has performed similar transplantation. Hoadley (**13**) has done some significant work in successfully grafting midbrain tissue in various parts of the chick.

Since the work on Amblystoma is too extensive to be noted in full, only a representative experiment by Detwiler will be described here. Detwiler transplanted the right anterior limb rudiment the distance of four body segments caudad to the normal position and under conditions providing for the regeneration of a limb in the new position. Under these circumstances the transplant will grow. Moreover, a limb will be regenerated from the site of the original transplant so that there will be two right anterior limbs. Detwiler (**8**, **9**) found that the movements in these two limbs are analogous and synchronous with each other and are also coördinated with the movements of the opposite anterior limb. The experiments by P. Weiss (44) confirmed Detwiler's results. He states that if the one limb effects a flexion at the knee, a flexion at the knee of the same intensity will be observed in the other limb; or if the one limb effects a dorsal flexion of the foot, a dorsal flexion is shown by the foot of the other limb. In recording this functional behavior, he has termed it 'homologous function.' The homologous function was observed regularly in all cases no matter what the topographical relation of the transplanted limb to the body and to the normal limb might be.

Detwiler (8, 9) has traced the innervation of the normal and transplanted limb. He finds that the transplanted limb does not receive all the brachial nerves (3rd, 4th, 5th), but the fifth nerve may be taken over in part or totally by the transplanted appendage. Transplanted appendages so supplied exhibit movements which are analogous and synchronous with those of the regenerated limb. When limbs are transplanted at greater distances they do not receive their innervation from the brachial nerves, and although they exhibit motility their movements do not synchronize with those of the other limb.

Hoadley (13) transplanted part of the mesencephalon of a forty-eight-hour embryo of a chick to the choria-allantoic membrane. An area from the last somites of the same embryo was likewise grafted adjacent to the mesencephalic tissue on the chorio-allantoci membrane. This somitic region was from one side and was free from the primordial nerve cord, so that any innervation of the parts would of necessity come from mesencephalic tissue. Under these conditions it was observed that when the somitic graft differentiated into striated muscle tissue, connective tissue, cartilage, and mesonephros, the nerves originating from the brain tissue extended and entered these masses. In other words, these nerve cells react to the differentiating and growing somitic grafts by growing to them and entering them, often traversing long stretches of the host mesenchyme in the course of their growth. In regard to the differentiation which occurs in these nerve cells, Hoadley states that it should be emphasized that the morphological differentiation of the cells was not altered visibly in these grafts, but that the cells bore the same marks of differentiation that characterize the cells of the normal mesencephalon. The function of these cells had been changed, however, for whereas in the normal development of the embryo they connected only with other nerves and formed only intermediate steps in the chain of transmission units in the reactions brought about from slight stimulation, in the grafts they ran directly to the end organ, and whether they were sensory or motor in function, they were in a sense correlative.

These cases of transplantation furnish clear examples that cells or tissue do not develop in isolation. They are dependent upon the effects of other tissue. In the case of transplantation of the primordia of the optic lens the optic vesicle determines the development of the lens. This is true if the vesicle itself is grafted into a new position. In the case of the transplantation of limbs of Amblystoma the outgrowth of neurones from the spinal ganglia is determined by the limb primordia. If the graft is in the neighborhood of the original site these nerves come from the brachial plexus, and the transplant and the regenerating limb on the original site are synchronous and homologous in function with each other and with the opposite limb. If the graft is at a distance from the forelimb, then the limb primordia 'attracts' the nerves from ganglia below the brachial region. In either case, the limb receives adequate innervation. In the case of the grafting of somatic and mesencephalic tissue on chorioallantoic membrane of the chick, the nerve processes reach out and innervate the somitic differentiations. In all these cases is exhibited an interdependence which the elementaristic theory does not clear up. In each case one tissue exercises a stimulating effect upon other tissues. Both Detwiler and Hoadley hold a dynamic conception of the factors causing the development of nerves and their growth toward their end organs. Hoadley says, "Hyman and Bellamy have shown for many forms that regions of high metabolism and rapid growth are electronegative externally to adjacent regions. It might therefore be readily assumed that regions in the grafts in which tissues are developing are of higher metabolic rate than those of surrounding mesenchyme. We would therefore expect to find the presence of an electric field in such a condition. Ingvar has found that the developing nerve cell reacts in a definite way to weak electric fields when grown in tissue cultures. Inasmuch as the response of the nerve processes given off by the mesencephalon is of a very general nature and an electrical stimulation is much more general than any substance produced by various kinds of tissues can be conceived of, it seems that the above experiments favor more generally some

dynamic theory of nerve attraction rather than a chemotaxis" (13, p. 180).

Detwiler holds a similar view, stating that it might be suggested that the differentiating limb rudiment represented a local region of high physiological activity and that, if according to Child, such regions are electropositive internally, it is conceivable that axones coming into the general vicinity are attracted by the electric field and electric polarization is brought about, much after the fashion in the developing nervous system according to Kappers and Child.

Thus the experimental evidence resulting from the various works cited goes to show that the neurone is conditioned by factors which are present within the developing organism. This evidence makes it highly improbable that the nerve cell is an independent structural unit of the nervous system. Morphologically and functionally it is a product of physiological factors at work in the organism. In this sense the organism is the environment to the cell or neurone, and to this degree the organism determines the polarity, the direction of growth, and the functioning of the neurone.

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THE EVOLUTION OF SOCIAL CONSCIOUSNESS AND OF THE SOCIAL SCIENCES

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The Priority of Social Objects

Experimental study of the child has shown that he has no definite perception of objects at birth. He must integrate all perceptions through the exercise of his senses. Gradually he acquires an awareness, however vague and incomplete, of himself as a behaving object more and more distinct from other behaving objects in his environment. Since he is relatively passive; that is, since his behavior is closely circumscribed spatially, and he must respond to stimuli or give stimuli to actively responding objects, usually human beings, he learns first to respond to human objects. To these he first gives definition while defining himself. His consciousness about them, his recognition or definition of them, is at first primarily emotional or attitudinal. That is, it is a social rather than primarily a physical, or a social and physical, consciousness which he first develops with respect to the outside world. And this outside world is made up primarily of persons rather than of inanimate objects, because he is not yet sufficiently self-active to bring many passive objects within the range of effective experience. People are moral before physical objects to him; and this they continue to be in large measure throughout his life. Even if he could learn to perceive them (as he cannot) in all their complexity as physical objects whose behavior could be predicted and manipulated on purely mechanical and chemical principles, he would probably never be able wholly and habitually to redefine them for purposes of self-control in physical instead of in moral terms.

This attitudinal definition of persons which the child applies to others he also assigns to himself. Although he is not without a rapidly growing sensory integration of himself, this

does not much enter into explicit and verbalized consciousness, at least in very early months. We know how slowly the child learns to see himself as a physical object. When he does accomplish this the achievement is primarily of the looking-glass self. He sees himself physically through the eyes of others. The taking of pride in his personal appearance, health, etc., comes as a reaction to the opinions of others. His attitudinal self-consciousness develops earlier and it is a reflection of the attitudes of others toward himself. This attitudinal and affective and social definition of himself is, in fact, the reverse of the same shield of his attitudinal or affective and social definition of others. He defines them in terms of the emotional experiences which they arouse in him and later learns to objectivate these emotional attitudes toward them in words, such as good, bad, pretty, ugly, kind, unkind, etc. As his attitudinal responses towards others increase in complexity and in discrimination of moral and aesthetic values his descriptive vocabulary, by means of which he objectivates the perceived sources of these attitudes, increases in intellectual content, until more and more of the perceptual description of the physical content and behavior of the object persons is mingled with the evaluation language by which he at first defines or describes them.

The passive objects, however, come in time to get a systematic verbalized physical or direct sensory definition in the consciousness of the child. This sort of definition begins vaguely and generally as early as the social or attitudinal definition, especially for those passive objects in closest contact with him, such as his clothing, the proximate parts of his crib, play objects, etc. Even the clothing and body of his mother and of other people whom he touches and his own body afford him sensory experiences to which he responds differentially. He experiences weight, pressure, hardness, softness, gentle and rough contacts, and sees colors, hears sounds and perceives objects with increasing differentiation of details and syntheses of recognition and adjustment meaning. But he does not necessarily verbalize these perceptions and thus build them systematically into his consciousness as physical objects. Normally the social attitudes and relations

first obtain organized objectivity for him, because they are more meaningful to him. But as he develops a wider range of behavior and of contacts, it is increasingly necessary for him to make use of physical objects as instruments to be used in securing the social adjustments and contacts he desires. He must crawl over the floor to arrive at a desired goal and he must manipulate various physical objects to secure attention and to call forth wished for responses. Other physical objects obstruct his attempts to secure social response and even give him serious pain. Because of this new and growing instrumental and obstructive significance of passive objects in his world he begins to depersonalize them of any attitudinal values they may have acquired previously because they were closely associated with active social objects or because they barred him (like the restraining hand) from such objects, and more calmly to analyze them intellectually and verbally into their sense-giving qualities in order that he may utilize them as instruments or remove them as obstructions. This process of control of passive or physical objects does not originate rationally or purposively, but it gradually develops into an intellectual verbalized process and thus comes to be the foundation of the physical sciences as distinguished from the behavior sciences, both biological and social.

Thus the child—and primitive man even more than the modern child, who early comes under the influence of physical science analysis and language—first defines himself and others in terms of subjective attitudes and extends this attitudinal definition to inanimate things. It is in this way that the first world of the child—and the world of primitive mankind—is a social, a moral, and affective attitudinal, world. We will now examine the process by which the limits of this primitively social world were cut down by means of

The Objectification of Persons and Things

Professor Cooley and other social psychologists have emphasized the bipolarity of self and social consciousness.[1] I

[1] C. H. Cooley, Human nature and the social order, especially chaps. 2–4; J. M. Baldwin, Mental development in the child and the race; Social and ethical interpretations in mental development.

have pointed out elsewhere[2] the essential dichotomy of all forms of social consciousness, the bi-polar character of all consciousness noted by Cooley being carried over into specifically social consciousness. Thus the social consciousness involving the me-other and other-me relationship is essentially more primitive, attitudinal and subjective, at least in its early development, than is the other-to-other type of social consciousness. While it is true that the other-to-other relationship can be grasped only on the basis of the me-other and the other-me relationship, as practice in the perception and definition of the more objective relationships grows, so likewise descriptive and analytical language grows to meet the needs of this more objective and abstract definition. And, in the course of time, self-consciousness either drops out entirely or remains unnoticed in this more objective phase of social consciousness.[3] This growing objectivity and abstractness of description and definition in social consciousness of the second order in time comes to reflect back upon the first or me-other and other-me type of social consciousness and even back upon self-consciousness itself to intellectualize and objectify its definitions. This is one of the important methods by which we are able, as we say, to be 'objective' instead of 'subjective' in our consciousness of our relations to others and about ourselves.

When we reach that phase of social consciousness which I have called public consciousness,[4] where we are concerned not with the concrete relations of two or more individuals whose behavior can be easily and concretely visualized, but with larger groups of people often not in concrete contacts with one another, it is necessary to find much more abstract language in which to characterize this public behavior and to

[2] L. L. Bernard, An introduction to social psychology, chap. 12.

[3] L. L. Bernard, An introduction to social psychology, p. 179; also Davis-Barnes, Introduction to sociology, pp. 468–475.

[4] My employment of this term differs from that of Cooley in that I conceive of public consciousness as the abstract consciousness of individuals when describing and defining the behavior of groups, publics, and other psycho-social processes, such as folkways, mores, institutions, etc., instead of the collective consciousness of a group of people attending to the same object of consciousness. See C. H. Cooley, Social organization, p. 10, and L. L. Bernard, An introduction to social psychology, pp. 179–181.

define the public entities thus behaving. Here the detachment and objectification of these social objects have become much more complete and thoroughgoing. The detachment of this sort of social (public) consciousness from our self-consciousness may be, and perhaps usually is, so complete that the behavior of men acting in such public relationships has only symbolic and abstract meaning for us.

However, this fact does not guarantee that our definition of such highly abstracted and objectivated behavior shall be in the mechanistic terminology of purely sensory or physical description we apply to physical objects. Here as elsewhere the psycho-physical description follows rather than precedes the personalistic attitudinal or moral-social and subjective description and definition. We still think of publics and institutions, of governments and traditions, of customs and beliefs, as good and bad, as fair and unfair, benign and brutal, and the like, even after they have lost in our consciousness all semblance of concrete personality. We recognize them in terms of attitudes, even of subjective and emotional attitudes, after the model of our early recognition of our mothers and playmates. Similarly, we think of ourselves in relation to these highly objectivated and abstract entities in the same affective and moral terms, as 'good' or 'bad' citizens, as 'faithful' and 'unfaithful' members of the church or lodge. If necessary we personify the invisible and abstract public control to make these moral affective and evaluative attitudes more real. Of course, that is what the primitive man always did. He had to have a personal divinity to give him his laws and 'revelations.' Hegel had his Zeitgeist and the Neo-Hegelian followers of Durkheim have their 'Something-over-and-above' which Allport has exposed but has not succeeded in extirpating.[5] Some of us still keep our 'instincts' and 'wishes,' our inherent 'virtues' and 'vices' as our modern substitutes for the ancient spirits which we are loath to abandon; and the primary-idealed, objectively attitudinalized 'average citizen' has to create a hero or a devil in every politi-

[5] L. L. Bernard, An introduction to social psychology, chap. 1, etc., and F. H. Allport, The group fallacy in relation to social science, *J. Abn. & Soc. Psychol.*, 1924, 19, 60–73.

cal situation, case of graft, or corrupt city government to explain concretely what he cannot perceive and state abstractly as a complex of social processes with perfectly naturalistic causes. Thus persist still in our day of abstract, objective and depersonalized public consciousness hero and devil worship and the fallacy of the 'good' and 'bad' citizen which William H. Allen exposed with so little result half a generation ago.[6]

But the process of objectivation and depersonalization of description and definition of social objects goes on slowly and increasingly in the social thinking of the race and in the social consciousness of the individual. The advantage of the transformation lies primarily in the fact that it substitutes an intellectual naturalistic description of processes and definition of objects for the old subjective attitudinal adjustments at a longer spatial and temporal range and, as we ordinarily say, controls the social adjustment process more effectively.

One development, itself a by-product of the process, which has helped tremendously in this process of objectification and abstraction of objects, is the concept of environments and the classification of environments. Environments came within our experience and became factors in naturalistic explanation as soon as man developed sufficient objectivity of description and abstract symbolic definition to grasp and relate a number of objects at the same time and in a single concept. The child has no appreciation of environment as a concept until long after he has objectivated persons. The concept of environment, whatever he may name it, comes upon him when he has learned to abstract and organize the consciousness of putative causal relations among objects external to himself. The process of organizing the concept of these external causal relationships not only gives body to the objective aspects of consciousness, but leads ultimately to science. At first these causal relationships are accounted for in subjective attitudinal terms and the philosophy of causation is that of personal will or magic. Primitive man's theory of causation was predominantly that of magic, which is bound up with the sub-

[6] W. H. Allen, Efficient democracy and universal training for citizenship and public service.

jective attitudinal consciousness and the attempt to make these subjective attitudes effective through wish or will, *i.e.*, as a physical objective extension of the subjective attitude, instead of through the operation of naturalistic physical or non-personal and non-attitudinal causes.[7] Only as man's consciousness comes to be based on discriminatory sensory perceptions and on concepts arising from these and as he learns to objectivate and describe and define objects and relations on this naturalistic sensory, as opposed to the subjective affective, experiences described above, does he pass from the realm of magic to that of science in his attempt to control the environment which he has created in his experience.[8]

The Evolution of Physical Objects

Effective objectification and detachment of objects could not be achieved until they could be described in terms of physical sensory analysis instead of in terms of subjective or affective attitudinal experience values. This process of depersonalization could most easily be effected in connection with those inanimate objects which we traditionally call physical and with the more passive living forms, the plants. Already we have noted that sensory experiences began approximately with or before birth, but the organized perceptual consciousness of these sensory experiences developed as secondary and contributory to the affective attitudinal definition and evaluation of social objects. It is probable that physical sensory definition would always have played this secondary and contributory role except for the fact of the existence of inanimate objects in man's environment to which he sustained a less affective attitudinal relationship and also for the necessity of stabilizing and objectifying the social environment by some system of objective and non-affective measurement of potentialities and relationships.

[7] L. L. Bernard, A psycho-sociological interpretation of magic, *Publ. Amer. Sociol. Soc.*, 1928, 22, 60–71. Children also go through a similar process of explanation in terms of magic, which is frequently intensified by fairy stories, nature-faking bed time stories, etc., and which is also sometimes minimized by an early contact with science instruction.

[8] L. L. Bernard, A psycho-sociological interpretation of magic, *Publ. Amer. Sociol. Soc.*, 1928, 22, 70.

It was the inertness of inanimate and unresponsive (vegetable) living objects which rendered it easier for man to depersonalize them. They were as we have seen personalized in the first instance mainly by analogy or as social instruments. The persistence of affective attitudinal conscious valuations toward objects after they cease to be merely adjuncts to persons depends upon an active reciprocal relationship, which could not be maintained with inanimate or 'mere physical' objects. The detachment of inanimate instruments from manipulating personalities with which they have been identified also became easier with this growing recognition of their relative passiveness and growth in sensory analysis of them as instruments. Thus the very process of the sensory analysis of instruments or tools which helped to render them more effective instruments of men aided in their ultimate intrinsic (not functional) detachment from men and depersonalization. When one begins to use, measure and weigh, shape and repair his tools, they inevitably become physical rather than personal objects, because such processes involve a high development of sensory analysis and definition. The fact that the bow and arrow which one has made may later be personalized through close instrumentalization and apparent readiness to adapt itself to the will and thus acquires a secondary affective attitudinal definition or characterization from close functional identification with its owner, does not militate against the principle of depersonalization through sensory analysis or psycho-physical control any more than the attempt of the sentimentalist and exhorter to improve social conditions through social magic or the 'good man' hypothesis invalidates the principle of the growth of objective or social science analysis of public relationships set forth above.

The function of language development in this process of depersonalizing inanimate or 'physical' objects is striking. The depersonalization is achieved in consciousness through the substitution of sensory descriptive definition for the old terminology of affective attitudinal characterization. Sensory analysis is accompanied by nouns and adjectives which serve to label the new psycho-physical distinctions developed

in the object. The increasing richness of language in such terminology not only bears witness to the growing importance of analysis as against affective valuation, but this terminology also becomes itself a part of the psycho-social environment and thus serves as a body of conditioners or controls of behavior capable of dominating and directing future responses to the physical rather than to the affective qualities of objects.

Relatively simple and passive objects are thus first objectified by descriptive sensory analysis, because it is easier to apply to them psycho-physical processes of measurement and comparison. In this process of sensory discrimination, not only are objects and parts of objects named and described in physical sensory terms, but standards or units of sensory discrimination or measurement are worked out which are also held fast and constant by the verbal labelling or symbolic description process. These sensory measuring units greatly simplify and facilitate future psycho-physical or sensory analysis and consequent objectification and depersonalization, for they constitute a universal language of the senses not less important than would be today a universal language for all mankind.

Another vastly important effect of this growth of sensory analysis and verbalization is that objects can now be not only defined but also objectively compared and arranged or classified with reference to one another. One meaning consequence of this fact is that it becomes possible for the individual to perceive and to behave intelligently with reference to a multiplicity of objects at the same time instead of perceiving and behaving with reference to only one object at a time. In other words, it results in the objectivation of environment and the institution of an understanding of environmental controls with a functional correlate of effective individual adjustment to a complex physical environmental situation. It reduces the unexpected and unaccounted-for responses by virtue of the increased perception and correlation of stimuli. This objectivation of an increasingly definite physical environment brings order into the world of human behavior and begins to drive the imputed animistic and spiritistic or subjective at-

titudinal out of nature. It helps to make nature physical instead of personal. Doubtless there have been many to deplore the decay of the poetic (mystical) content which is supposed to accompany this transformation of our world, but the compensations of increased security and control over life have abundantly recompensed the loss of these 'poetic values.'

The objectification of the social environment comes more slowly and with greater difficulty, because those objects we call 'social,' that is, the more active and unstable animate and attitudinal objects, are less easily detached from ourselves and from an evaluative significance. Because of their dynamic character they are reciprocally related to us in much higher degree than are the so-called 'physical' objects mentioned above. This means that our social consciousness of them is closely tied up with our self consciousness and we respond to them with and characterize them in terms of affective attitudes. Where the affective response is uppermost the sensory description lags behind. Also, the greater complexity and more rapid transformation of the behavior of these social objects with reference to us renders it impossible for us adequately to describe them and their behavior in terms of relatively static psycho-physical or sensory terminology. Even if he could have paused to make psycho-physical observations and measurements upon people—as at last we are beginning to do—the mere physical statement of the results of these sensory analyses could not have given the primitive man, as they could not afford the child, an adequate functional understanding of men and of their behavior. When his relationships are with groups, publics, abstract psycho-social processes, and institutions, that is, with men collectively instead of individually, the task of sensory analysis and description of his social objects is absolutely impossible. It was necessary for him to await the development of highly abstract or conceptual methods of integrating sensory perceptions before he could describe social objects abstractly. These abstract relationships must be conceptualized, which means verbalized and labelled in some sort of generalized or informal statistical manner. That is, new and higher units of psycho-

physical measurement had to be developed to describe collective behavior or social environments as wholes. And this generalization and conceptual verbalization of higher and more abstract units of measurement could come first most easily through its application to the simpler 'physical objects.' For this reason the objectivation and definition of social environment in terms of definite descriptive terminology has followed that of the physical environment. To be sure, there was an objectivation of the social environment on an affective attitudinal basis which posited a supernatural relation of will instead of a naturalistic relation between the individual and the environment, and which persists into the present in the form of magic survivals, mythology, theology, mystical philosophy, vitalistic biology, and the like. Space does not permit of the further consideration of this line of development here.

The Priority of the Physical Sciences

This objective description and definition of 'physical' objects in terms of sensory discrimination is the beginning of physical science. The completion of physical science on the descriptive side is the objectivation of a definite environment for each defined physical object. This involves the description and definition of the object primarily in terms of its physical relation to other objects of its class and of related classes as well as in terms of the psycho-physical or sensory perceptive responses to them, secondarily and derivatively in terms of the relations of these objects thus physically defined to one another. Physical science thus has its perceptual or individual sensory response aspect and its objective or environmental aspect, which is derivative.

These two aspects of scientific definition and classification are now well advanced with respect to the inorganic and lower organic worlds. Here depersonalization is about complete. 'Personality' qualities have all but disappeared from such objects. Controlling spirits have been detached from them, and now the physicists and chemists are analyzing and measuring away the last vestiges of those 'essences,' 'entelechies,' and other essential principles and qualities which succeeded

the spirits in the control of physical objects. In their place they are substituting mathematical formulas which merely state coincidences and sequences of phenomena. Attitudinal definitions of physical objects long ago were replaced by descriptive psycho-physical definitions, as indicated above. The attribution of moral qualities has given way to a purely naturalistic interpretation. Likewise, the concept of the physical environment has been extended and organized until we now speak of the physical and chemical worlds, by which we mean that there are groups of physical phenomena, which are capable of explanation without reference to personality phenomena. They are conceived of as independently organized wholes. This concept of the physical-chemical world or environment has been extended to embrace the whole of our universe and the naturalistic concept or way of thinking has come to dominate our attitudes toward and explanations of it. Personality, which was once the dominant concept in the universe is now in process of yielding first place—has in fact, yielded it—to the physical or naturalistic concept or way of thinking, although the former has by no means completely disappeared from the minds of all men.

Biological objects are being objectivated and depersonalized by the same processes. Their description and definition is made increasingly in terms of physics and chemistry and the description becomes more and more quantitative, which is the highest phase of psycho-physical or sensory measurement and definition. Spirits, for awhile replaced by metaphysical essences, entelechies, élans vitals, and the like, are now practically wholly superseded by mechanistic interpretations. The biological objects are thus able to assimilate to physical objects and the biological environment to the physical environment. Accordingly there begins a hierarchy of objects and of environments with the same general unitary descriptive or scientific formula instead of under different and conflicting formulas. This marks the beginning of the unity of science, the perception of which in the nineteenth century was hailed with triumph by Comte, Spencer, Haeckel, Ward and other monists. Biological science has now emerged as a

phase of universal science. The moral and affective attitudinal characterizations of the biological world disappear in myth or trail in the personifications of bed-time stories and nature-faking lore for children and a few scientifically immature grown-ups.

Human behavior is the last stronghold of the field of affective attitudinal definition and subjective interpretation of phenomena, the last to be invaded by the objectivity and descriptive sensory measurements first developed by physical science. But objective physical, chemical and biological measurements and descriptions are being constantly and increasingly applied here also, as a means to the better understanding of the human world and of securing individual adjustment to it. Spirit domination gave way to psychic and moral qualities with the decay of witchcraft and demoniacal possessions. The virtues and vices, instincts and innate conscience, sentiments and 'central' and underived emotions, were the legitimate successors to these spirits. We are now discarding such innate and underived categories as these also and are going back to biochemistry and biophysics for deeper insight into our problems of human behavior and adjustment. We are searching for a purely objective analysis and explanation of behavior. Introspective description must be checked and corrected. Perhaps this transformation from magic to science will also grieve some because it removes still more 'poetry' from life, but the coming generation will probably willingly exchange the 'poetry' dependent upon mystery and uncertainty for security and certainty and control. New generations almost always have been willing to make such an exchange. Such transformation does not, as some appear to think, threaten the elimination of mind from our world and leave us like 'sticks and stones,' but defines mind, refines it and purges it of magic and superstition, and makes of it an effective instrument of adjustment.

Along with this de-spiritization of human personality and the substitution of naturalistic for magical explanations of human behavior has also gone an attempt to create a naturalistic human environment for individuals. The earlier social

environment, as we have seen, was a supernatural one and its relation to man was that of affective attitude and will. This supernatural and personalistic environment was gradually replaced, historically, by a metaphysical one of universal essence, forces, natural law, etc. Verbal remnants or fossils of this metaphysical theory of environment still persist in social philosophy in such expressions as social forces, instincts, wishes, essential qualities, etc. A naturalistic theory of social environment based on the analysis and classification of interaction and inter-adjustments of persons and groups is now appearing with the growth of a science of sociology, and is being classified. As social science finds its place in the hierarchy of natural sciences the social environments also relate up to the antecedent environments and take their place in the hierarchical classification.[9] On the bases of these naturalistic developments in human personality and in the social environments the social sciences have been or are being integrated.

The Delayed Appearance of Social Psychology

Not only did the social sciences receive formulation and verbal embodiment after the physical sciences were well under way as sciences, but there has been some regularity of sequence in the appearance of the several social sciences themselves. Those dealing with human behavior as conditioned by physical environment necessarily were integrated first, because their phenomena were first described and their objects and environments were first naturalistically measured and defined. Thus anthropogeography, economics, archeology and social geography arrived late in the eighteenth and in the nineteenth centuries. The social sciences concerned with men in objective groups and institutional relationships, whose phenomena fall in the second phase of dichotomous social consciousness, came into orderly verbalization or systematic

[9] For one attempt at a statement and classification of these environmental data and relationships see L. L. Bernard, A classification of environments, *Amer. J. Sociol.*, 1925, 31, 318–332; An introduction to social psychology, chap. 6; Ogburn and Goldenweiser, The social sciences and their inter-relations, pp. 353–361; Davis-Barnes, Introduction to sociology, pp. 410–419.

statement in the second place. Thus critical history evolved from literary narrative and metaphysical speculation into a body of scientifically measured and tested data, properly catalogued and classified, early in the nineteenth century and was pretty well perfected in these respects by the end of the nineteenth century. Constitutional history, which grew out of institutional history, followed later, and anthropology, demography, business administration, political administration and the description of political and mass behavior, the collective psychology of the Bagehot, Tarde and Le Bon type, were successively integrated. Each of these social sciences, and others similar to them, began to be integrated in method and basic content before the beginning of the twentieth century.

Those social phenomena concerned particularly with psychic interaction and reciprocal attitudinal behavior are the most recent to develop scientific definition and correlation. In fact, they are even now in process of receiving their formulation as science content. Thus, what might be characterized as psychic interaction sociology [10] and social psychology divested of the Neo-Hegelian oversoul hypothesis and the metaphysical notions of instinct and entelechy, have been organized by men still living and for the most part still active as teachers and investigators. The still more subjective attitudinal phases of behavior, usually spoken of as ethical, aesthetic, and religious, have yet to be successfully described and defined and organized objectively and thus erected into sciences. Hopeful beginnings have been made even here, especially on the descriptive side, in books and papers dealing with social ethics and religious phenomena.[11]

The late arrival of these highly attitudinal social phenomena at the organic consistency and objectivity of science has been due to a number of discernible circumstances. Important among these is the difficulty of making objective descriptions

[10] See, *e.g.*, C. A. Ellwood, Sociology and modern social problems, p. 16, and Park and Burgess, Introduction to the science of sociology, p. 342.

[11] See, *e.g.*, C. A. Ellwood, Christianity and social science and The reconstruction of religion; V. Branford, Science and sanctity; S. Mathews, Contributions of science to religion; R. A. Millikan, Evolution in science and religion; J. A. Thomson, Science and religion.

of these phenomena and at the same time leave out spirits, essences, innate qualities, integral faculties, such as free will and the like, as well as other fiatistic and other unstandardized and unmeasurable factors, and reduce behavior to a naturalistic explanation in objective terms instead of in terms of subjective attitudes which cannot be measured accurately and recorded objectively. It must not be forgotten that science is always a form of language, a method of definition and of description, not of feeling or of overt behavior. There is also the kindred difficulty of making the substitution of an objective description of the phenomena of human behavior for subjective moral evaluations of it. Social science has not arrived in any field until moral and affective attitudinal evaluations of phenomena have been reduced to secondary rank, to be applied only after the objective analysis has been made and only in the light of the broadest objective view of the complete social organization and environment. This does not mean, of course, that moral and aesthetic evaluations have no place in social control or that they cannot be studied scientifically as social phenomena, but it does suggest that they defeat their own ends when they are allowed to get in the way of purely objective scientific analysis and definition.

There are also other difficulties, such as that of detaching the consideration of social events in which we participate or may participate or have participated from the dominance of our own self-consciousness and of considering them wholly in the light of the second or objective phase of social and public consciousness.[12] It has also been difficult for society to achieve collectively a descriptive vocabulary and language sufficiently abstract and comprehensive to cover these highly complicated and abstracted relationships of groups, publics, psycho-social processes and institutions as component phenomena instead of as artificial personified entities. A correlative difficulty is that of measuring quantitatively and of differentiating qualitatively these complex abstract phenomena. Here statistics must largely replace geometry and the calculus substitute for arithmetic, as used in the physical and

[12] L. L. Bernard, An introduction to social psychology, pp. 177–183; Davis-Barnes, Introduction to sociology, p. 471.

lower biological sciences. For here we are describing and we are measuring not merely concrete simple compact physical objects and space relations, but abstractly integrated spacially discrete objects and relationships, that is, people with changing characteristics and attitudes behaving in ever fluid relationships. Quantitative and qualitative values never remain constant for such phenomena as they do in the case of a stone or a chair. The method of description and definition in the psycho-social sciences is, therefore, super-physical and super-sensory, but not super-naturalistic and subjective.

Evidences of the rise and definition of the psycho-social sciences are, therefore, numerous. There has been a transition from subjective attitudinal characterization to objective naturalistic description and definition of phenomena in psychological sociology and in social psychology, and in less degree in other psycho-social phenomena. There has been a growth of the social adjustment or functional concept at the expense of the concepts of revealed and metaphysical moral qualities in ethics. There is gradually occurring an elimination of spiritistic concepts from the psychology and sociology of religion and the substitution of a naturalistic account of the operation of ethical and aesthetic attitudes in social control. Finally, there has appeared the concept and classification of the psycho-social environment [13] which gives positive and functional significance to all aspects of behavior in social situations by relating these up naturalistically to the whole complex of psycho-social phenomena and ultimately to the whole hierarchy of all of the environments which operate directly and indirectly to set the conditions under which human behavior, especially in social situations, must occur.

To summarize, it is apparent that the science of social psychology is, therefore, near the last of the social sciences to arrive, following at varying distances after the physical and chemical sciences, biology and the older and more depersonalized and physically more adequately grounded social sciences. This tardiness of arrival has been due to the fact that it was necessary first to develop a technique of objectification and measurement in the simpler and more passive fields of be-

[13] See footnote 9.

havior before this could be done in the more immediate and complicated and active phases of experience and thus make possible the substitution of an objective descriptive definition of social objects for the subjective attitudinal definition of them. As a correlative process to this development of objective description and definition of social objects, and their organization into a body of verbalized experience called a science, went also the objective organization of their actual and potential relationships or correlate and antecedent experiences which we may call environment. In the case of human behavior this environment is exceedingly complex and extends back to the other fields of objectivated experience, even into the realm of physical contacts, constituting an extensive hierarchy of environments. Although social or attitudinal consciousness arose before well-defined physical consciousness, the physical sciences, for the reasons stated above, necessarily preceded the social sciences and particularly social psychology.

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ALTERNATIVE RESPONSE AS A FORM OF "SUBLIMATION" [1]

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'Changing the subject,' switching attention and activity, when such change occurs from a lower to a higher form of activity, is sometimes interpreted as 'sublimation of the libido.' From the psychological point of view, however, a more useful explanation must derive from metabolic and neural processes. Certain of these processes will be suggested here together with some relations between the outward phenomena.

Upon increased arousal of any function, there occurs an increased energization of that function through an enhancement of the local metabolism, along with changes in resistance of the reflex pathways involved.[2] In other words, there is a measure of metabolic process, in the sense of delicately poised circulatory and other physiological processes, which can function now here and now there, to meet the emergencies and special calls for development of the different organs of the body; processes which can aid the production of saliva or the production of sex secretions, for example, as required. To this extent we can agree with the libido theory; but this is quite different from saying that such shift of development, with its accompanying change of attention, represents a transmutation of the activity peculiar to one function of the body into the output of some other function or functions. It is also very different from supposing that such basic diversion of energies can take place without limit, and always to the profit of the organism as a whole.

A second large explanatory factor for shift of activity

[1] Read at the Twenty-first Annual Meeting of the American Psychopathological Association held at Boston, Mass., May 29, 1931.

[2] *Cf.* K. S. Lashley, Physiological analysis of the libido, PSYCHOL. REV., 1924, 31, 196.

will concern us more particularly here. This factor we may call simply 'alternative response.' It is the process by which, given any two patterns of response, one pattern gains the right of way, while the other is completely or almost completely inhibited.

In its simplest form, this process appears in 'reciprocal innervation' (Sherrington), in which, for example, contraction of the flexor muscles in a limb is accompanied by relaxation of the extensor muscles in that limb.[3]

The forms of alternative response which most obviously relate to the present study seem to fall into five general categories, as follows: If any impulse or tendency is balked, there may ensue an alternative arousal of (1) various striped-muscle patterns; (2) the emergency emotions; (3) the mechanisms of laughter and crying; (4) appetitive functions; and (5) thought processes;—with various gradations and complications of these.

The evidence for such a general classification is, in brief: (1) The diffusion of striped muscle response when, for example, an animal's scratch reflex is balked. (2) The arousal of the 'emergency emotions' in those situations for which the animal's cerebro-spinal neuromuscular system is not adequate at the moment. (3) The apparent dependence of laughter and crying upon interference with other response tendencies. (4) The way unhappy individuals may resort to thumb-sucking, masturbation, and other activities of food and sex. (5) The occurrence of thinking when all our more primitive responses prove inadequate.[4] In other words, if ever we are in a state

[3] *Cf.* C. S. Sherrington, The integrative action of the nervous system, New Haven, Yale University Press, 1906; and W. McDougall's suggestive conception of 'inhibition by drainage,' Outline of psychology, New York, Scribner, 1923, 179n., 280.

It is true that reciprocal innervation seems to differ from the further forms of alternative response about to be described in that in reciprocal innervation one set of muscles *relaxes* as the other set of muscles is *contracted*, while in the further forms of alternative response one pattern *functions* in lieu of another pattern that is *inhibited*. At root, however, the processes may be the same, since inhibition of any pattern may involve initial stimulation followed promptly by more or less complete relaxation of that pattern, which relaxation is then maintained by the concurrent activity of the alternative pattern. *Cf.* R. Dodge, The problem of inhibition, PSYCHOL. REV., 1926, 33, 6–7, 174, 186–187, 9–10.

[4] Convenient references are (1) A. I. Gates, Elementary psychology, New York, Macmillan, 1925, 66. (2) J. Dewey, J. R. Angell, and others, as cited in W. S. Taylor,

of perfect equilibrium, for so long as that state lasts we neither exercise, nor become emotional, nor laugh nor cry, nor indulge the appetites, nor think; but when we are balked, we may follow some one or more of these modes of response, apparently in lieu of the other modes (within certain limits).

The frequent complications between the several types of reaction need not detain us, other than to acknowledge them; since our main interest in such variety of response is in the degree to which any of the types of reaction can be substituted for other types. Fear and anger may occur along with crying, or at least in rapid alternation with crying; the crying may be accompanied by angry gestures; restless activity of the limbs may be associated with thinking. Yet daily life provides a number of examples in which the substitution of one mode of response for another seems very complete.

The twelve-months-old baby whose right hand is caught and restrained from playing the piano may play cheerfully with the left hand, while the captive right hand lies relaxed.

Readings in abnormal psychology, etc., New York, Appleton, 1926, 717–718; W. B. Cannon, Bodily changes in pain, etc., New York, Appleton, 1915; F. H. Allport, Social psychology, Boston, Houghton Mifflin, 1924, 93–94; also Spinoza and other philosophers. (3) G. W. Crile, The origin and nature of the emotions, Philadelphia, Saunders, 1915, and Man—An adaptive mechanism, New York, Macmillan, 1916. (3–4) Allport, *op. cit.*, 255 ff. (5) C. J. Herrick, who says that in the higher mammals, when "the rapid preformed reflex mechanisms fail to give relief . . . there is a stasis in the subcortical centers. Meanwhile the higher neural resistance of the cortical pathways has been overcome by summation of stimuli and the cortex is excited to function. Here . . . by a process of trial and error, perhaps, the elements necessary to effect the adaptive response may be assembled and the problem solved" (Introduction to neurology, Philadelphia, Saunders, 1916, 307).

Interesting in this connection are the types of popular 'philosophy' which correspond quite well to these general types of alternative response. Many young men come under the influence of the gospel of muscular exercise: 'If you are down in the mouth, or if you have a grouch on, work it off in the gymnasium'; the common prescription, too, for the relief of sex pressure. The cult of the emergency emotions has not become so self-conscious, apparently; yet many of us have known people who hold that to get 'good and mad' occasionally is good for the soul, and who think that any one who does not do this is somehow lacking in vitality. Laughter and crying, as remedies for spiritual ills, have a wide acceptance, running all the way from the popular notion of 'a good cry' to the more elaborated philosophy of Pollyanna. The appetitive functions have their devotees. Thinking, as a prescription, is likewise known variously to the popular mind; but for a real appreciation of this mode of adjustment we have to read the Stoical philosophers, especially, Spinoza.

Compare, however, the discussion in the text, below.

The small child, playing happily but quietly on the floor with toys, when put into a snug-fitting sleeping bag on the porch is transformed forthwith into a happy vocalizer whose repertory can be heard for a block in every direction. Random movements of the legs relieve, for a time, the restlessness that comes from bladder pressure. An excess of joy may be taken care of by jumping up and down, by laughter, or by crying. Pain may be reacted to in a similar variety of ways. The angry person who feels helpless in his rage may throw himself into whole-hearted weeping. Anger may also be 'worked off' on a woodpile or a punching bag, or through hysterical laughter, so long as the stimulus for the anger does not recur; or the subject may think hard about real or fictitious means of adjustment of the angry situation. Conversely, much movement of the limbs can occupy the subject's attention as a substitute for thought.

Evidently some forms of alternative response are native, or are so little dependent upon learning as to be inevitable in the child's development. Long-muscle diffusion, activity of the emergency emotions, and laughter and crying are in this category. There must be a native tendency, too, for the organism to resort to thought processes, when other patterns fail; for otherwise thinking would never occur. Thought patterns, on the other hand, are largely if not wholly learned. The degree however to which any of these ways of reacting is used as a substitute for others is significantly modifiable by learning.

At one time the writer worked in a business office with a large man who, if ever his activities were interfered with, if say his desk drawer stuck, would break forth into random pulling, pounding, and stamping, also thunderous language, which would shake the building more or less. Circulatory changes resulting from emotional activity were evident, too. In striking contrast was the experience of calling on a little woman, aged about fifty years, at another office for some document. Waiting in an anteroom, the writer heard a series of sounds which he finally recognized as laughter coming from the inner office. Thinking that perhaps he had been expected

to come in there, he stepped to the door, and discovered that the laughter was the woman's way of reacting when her desk drawer stuck.

The difference between the two cases may have been innate, of course; but it would seem possible that, given the necessary training in childhood, the choleric man could have become so organized as to laugh uproariously upon any similar occasion. The Japanese and Chinese, according to travelers' reports, have cultivated the art of laughter in just this substitutive way. "If you start to sympathize with a Japanese for the loss of a relative," says one, "you are met with a mirthless laughter. When an accident occurs, like when a friend of mine was spilled from a rickshaw and badly cut, the onlookers laugh, not from malice, but in recognition that something serious has happened. (Malice is expressed in quite a different way, by spitting on the person, or if they do not dare to do that, then by spitting on the ground near his feet.)"

Another observer, who lived in Japan for several years, offers the following as typical: "A friend of ours, a man to whom I had been giving special lessons in English, called one afternoon to invite us to the funeral of his little daughter. We did not suspect this at first, though, because he began with small talk and somewhat hysterical laughter. When he told us his child was dead he laughed again, and after asking us to come to the funeral he went away laughing. At the funeral, we noticed that he moved about among the guests, offering them cigarets, and smiling and laughing. It looked as though he wanted to make everybody happy." [5]

In our country, learned substitution of response is observable in many forms. "One case," says Hall, "carries a bit of wood in his vest pocket and bites it when he begins to feel the aura of temper. . . . Many go off by themselves and indulge in a luxury of expressions they want none to hear. . . . Girls often play the piano loudly, and some think best of all." [6]

[5] Observations by Professor H. R. Cobb of Smith College and by Mr. F. W. Brown of the National Committee for Mental Hygiene. The former suggests that the laughter thus cultivated might have been motivated originally by a desire to avert the demons.

[6] S. Hall, The Freudian methods applied to anger, *Amer. J. Psychol.*, 1915, 26, 439–443; E. S. Robinson and F. R. Robinson, Readings in general psychology, Chicago, University of Chicago Press, 1929, 597.

We should suppose that the involvement of cortical activity must be peculiarly subject to learning, since this form of substitution is central to any education in self-control. We *learn* to think before we speak or act impulsively.

When it comes to alternation between *acquired patterns*, the role of learning is most obvious. The child walks *or* skips; the youth dances a two-step *or* a waltz; the acrobat runs on his feet or on his hands; the student studies his history or his mathematics, and not both at once; Socrates decides whether to escape from his prison or to drink the poison hemlock, and drinks the hemlock without apparent muscular tension to the contrary;—these alternations occur because the several patterns of response have been so well learned and integrated into the total system of behavior that they can be touched off to the exclusion of certain other patterns. When the patterns are not thus well learned and integrated (normally the two processes go together), the child is liable to walk skippingly, and the youth to waltz in the two-step manner, if at all; the student's mathematics will block his history; and Socrates would have been inhibited in his martyrdom. Partially assimilated activities are naturally subject to conflictful and unadaptive arousal.

The point is brought out by Hamilton in behavioral terms. "Selectivity of adjustive function," he says, "implies possession of some sort of capacity for inhibiting dysteleologic adjustive impulses; and however we may explain inhibition as a physiologic process, we cannot doubt that this process extends, in cases, to a reduction or even a total elimination of receptivity to various kinds of stimuli. An animal may be so trained (*e.g.*, the dog in Pavlov's laboratory) that, in spite of positive innate or acquired tendencies to react in a particular way to a particular stimulus, it will finally cease to make an appreciable direct response to that stimulus. Any person who has ever trained a hunting dog to follow only the fox scent will accept the accuracy of this generalization. The young foxhound will first respond to a rabbit scent with free trailing movements, then, as his training progresses, with abortive trailing movements, and, finally, if he qualifies as a good

hunter, he will make no observable response to rabbit scents. Observation of his behavior while he is being trained suggests that at first inhibitive interference with the reaction-arc connections for rabbit-trailing occurs near the effector end of the arc, but that as his training progresses this interference moves backward, as it were, toward the receptor end. When a dog is so well trained that he responds overtly to the fox scent alone while hunting in a field or thicket where rabbits abound, it seems likely that the first synapse beyond the receptor end of the arc for rabbit-trailing is not formed" or traversed by the nervous impulse.[7]

The same sort of 'moving backward' of interference has often been observed in human subjects under laboratory and other conditions. Only the trained typist can shift readily from one style of keyboard to another. As Jersild observes, "the evidence indicates that just as a given mental set comes into being through practise so also a more comprehensive mental set, calling for shift between activities which taken separately involve mental sets of their own, can be formed through practise. After more or less practise, depending upon the activities involved, this more comprehensive set will function just as effectively as the mental sets of a more simple character." Furthermore, "the ability to shift should be regarded not so much as an isolated factor as an intimate correlate of the general type of ability involved in all response situations." [8] We must conclude that Socrates drank the hemlock easily because that pattern of response, with all that it meant, was organized with his total attitude toward life.

Now an important result of such alternativeness of response seems to be that the unused alternative patterns, because inhibited before their arousal, so to speak, do *not* persist as 'balked dispositions' which press for 'sublimation.' The question is, then, How far can alternative response apply to relieve the tensions of the organism?

[7] G. V. Hamilton, An introduction to objective psychopathology, St. Louis, Mosby, 1925, 273–275. *Cf.* such studies as G. Schwarz, Über Rückfälligkeit bei Umgewöhnung, *Psychol. Forschung*, 1927, 9, 86–162; and those mentioned in A. T. Jersild, Mental set and shift, *Arch. Psychol.*, 1927, 89, 7–11.

[8] Jersild, *op. cit.*, 58, 79.

Unfortunately for the adjustments of some people at some times, there are limits to the process. Alternative response is most effective, probably, in those situations for which the motivation is either a general condition, such as adrenin in the blood, or an intention, such as to take exercise or to solve a given problem, for which one of two or more response patterns may be adequate. Yet even here some limitations appear. We do not know, for example, that 'laughing off' the adrenin in the blood (through the chemical changes that go with any exertion) is as *economical* as is 'working off' with the long muscles. And as for intentions, we observe that whenever the intention to exercise gets set on a specific form of exercise, or the plan to solve the problem turns into a determination to solve it by a particular method, much alternative response becomes inapplicable. Thus in many of us, disappointment at being prevented from picnicking on Pushaw is incompletely assuaged by picnicking on Chemo; if Chemo is out of the question, and we have to stay home and pitch horseshoes instead, the undercurrent of disappointment is stronger; and if recreation of any sort is prevented, the urge to break away from routine becomes acute and persistent.[9]

Furthermore, a great many of our urges are vitally dependent neither upon general physical conditions nor upon goal-ideas, but derive from specific intra-organic stimulations which must stimulate until relieved by specific circumstances. In this group come all the basic life-maintaining urges of hunger, thirst, respiration, circulation, heat, cold, pain, fatigue, elimination, probably other organic conditions including 'restlessness or the need for action,' and certainly a vital part of 'sex.' [10] These urges are relieved only, so far as we know, by the specific provision of nourishment, liquid, air, blood supply, and normal temperature, and by the removal of the causes of the pain and of the internal conditions of fatigue and the other organic states, including, we have

[9] *Cf.* the basic study of intentions by B. Zeigarnik, Das Behalten erledigter und unerledigter Handlungen, *Psychol. Forschung*, 1927, **9**, 1–85.

[10] List of urges largely from J. F. Dashiell, Fundamentals of objective psychology, Boston, Houghton Mifflin, 1928, 234ff., and M. W. Curti, Child psychology, New York, Longmans, 1930, 292ff.

every reason to believe, the removal of the internal stimulation to sex activity.[11]

The fact that the urge to sex activity can be removed or reduced by responses during sleep, erotic daydreaming, 'spooning,' and the like, though significant for social practise, is no support for the theory of sublimation. On the contrary, the way these processes seem to relieve sex irritation in proportion as they release specifically sexual responses, is strong evidence against the theory of sublimation, and leads to the practical implication that for the conscientious individual to strive to 'transmute' an organic nucleus of sexuality may mean excessive nervous strain as well as self-deception. (This is, of course, no argument for license. Having acknowledged a given urge quite biologically, the individual has before him the problem of adjusting all his interests so far as possible, not satisfying some through destroying others. A decrease of life through self-denial in one respect may be necessary at times for maximal self-expression in other respects, in view of social conditions.)

Thus the psychology of shifts of activity seems to reduce to adjustive metabolism with alternation of response; and these processes, though very important for behavior, reveal definite limitations in use, and point to basic inadequacies in the theory of sublimation.

[11] Pavlov's observation that he could condition a dog to respond to a noxious stimulus with benign salivation does not establish alternative response as cultivable without limit. He found that there is a limit to such contrary conditioning; and when that limit is exceeded—when say the noxious stimulus to which the dog has been trained to respond benignly becomes too strong—the dog suffers a nervous breakdown. I. P. Pavlov, Conditioned reflexes, New York, Oxford University Press, 1927, 302 *circa*.

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OUTLINE OF THE CONDITIONS UNDER WHICH LEARNING OCCURS

BY CARL NEWTON REXROAD

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Unquestionably, one of the major tasks of psychology is to discover and state the conditions under which learning takes place. Progress in this task is impeded by lack of a clear-cut definition of the task. Carr [1] has recently pointed out that there is much confusion because the task of determining the conditions under which associations or new stimulus-response relations are developed is not kept distinct from the task of predicting which of several associations or responses of an associative- or habit-system will occur. There is also the confusion resulting from the fact that some psychologists use the development of new mental associations as their criterion of learning while others employ the formation of new stimulus-response relations. Again, there is confusion because of the failure to separate the essential requirements for learning from contributive factors. We will attempt an outline of the conditions required for learning, an outline as free as possible from these various sources of confusion.

Definition of Learning

To begin with, learning may be defined as the changes in certainty with which particular activities occur under given stimulation conditions when those changes arise in the course of the organism's experience with the stimulating conditions. Activity is to be understood as denoting any process occurring within an organism, whether the process is physical or mental or both. By certainty is meant the probability that the activity will take place under the stimulation, and the measure of certainty or degree of probability may be the regularity, the readiness, or the vigor with which the activity occurs

[1] H. A. Carr, The laws of association, Psychol. Rev., 1931, 38, 212–228.

when the stimulation is present. The change in certainty may be either an increase or a decrease, a greater certainty or a lesser certainty; or from the standpoint of the stimulus, the change may be a gain or a loss of effectiveness for a particular activity.

Types of Learning

On the basis of the nature of the change, four types of learning are to be distinguished. (1) Simple conditioning, in which a stimulus (simple stimulation) gains effectiveness for a given response. (2) Negative adaptation, in which a stimulus loses effectiveness for a given response. (3) Substitution of response, in which a stimulus loses effectiveness for one response and gains effectiveness for another. (4) Acquisition of coördination or the development of skill, in which a situation (complex stimulation) loses effectiveness for certain activities and gains effectiveness for several activities executed simultaneously or in sequence.

In the first three types both stimulus and activity are relatively simple, while in the fourth both are complex. In the first three the activity after learning is essentially the same as before learning, while in the fourth there is a marked difference in the activity displayed before and after learning. This, however, does not imply that the elements of the complex skill are created in the process of learning. Rather, they are synthesized or coördinated into a complex act. Originally the situation evokes many relatively disjointed responses, and in the process of learning some of these are completely eliminated while others become more readily evoked and become joined together into one unified whole act. This synthesis comes about by virtue of the attaching of each response in the act to the constant features of the situation and to stimulation derived from the preceding response, *i.e.*, it is an outcome of the readiness with which the situation arouses specific responses. Whatever the type of learning considered, the fundamental change is, then, a modification of the certainty with which certain responses are produced under particular stimulating conditions. The attempt to discover the laws of learning consequently becomes the attempt to discover

and state those conditions which give an increased effectiveness of stimuli as opposed to those which result in a decreased effectiveness. These differential conditions will be the ones fundamental to learning.

Conditions for the Gain of Effectiveness of Stimuli

Experiments and observations which led to the formulation of the associative law of contiguity, of Thorndike's law of use, and of Pavlov's first principle of conditioning, can be generalized in the single statement that *a stimulus gains effectiveness for a given response when the stimulus is followed by that response as a dominant response.*[2] The ringing of a bell gains effectiveness for salivation when the ringing of the bell is followed by salivation as a dominant response. (Whether or not a given activity is dominant is to be judged either by the experimenter who is observing behavior or by the person who is acting. In the latter case a dominant activity is one which is represented in the focus of attention.)

This statement avoids the misunderstandings arising from the more conventional statements in terms of contiguity of things to be associated or of temporal contiguity of conditioned and unconditioned stimuli. The conventional statements put the emphasis upon what is outside the organism rather than upon what takes place within the organism, and everyone realizes that activity within the organism is essential to learning. The present statement calls attention to the organism's activity, and (what is more) it states that if an activity is to become attached to a stimulus, the activity must be dominant. Much of the debate about the efficacy of frequency rests upon the failure to specify what it is that is frequently related, whether it is two stimuli, a stimulus and a subordinate activity, or a stimulus and a dominant activity. The frequent ringing of a bell when a satiated dog is presented with food is ineffectual for producing a conditioned salivation. (Probably Pavlov has never felt the need of reporting this obvious truth, yet it shows the absurdity of speaking only of the pairing of conditioned and unconditioned stimuli.) The

[2] *Cf.* C. N., Rexroad, General psychology for college students, p. 154.

frequent ringing of a bell when a dog secretes saliva in response to acid in the mouth is slightly if at all effective, for the salivation may well be only a subordinate part of the dog's activity. But the frequent ringing of a bell when a hungry dog is salivating is effective, for then the salivation is a part of the dog's dominant activity. In like manner, the frequent presentation of a memory series to a person who is engaged in conducting a memory experiment is ineffective in producing memorization, but the frequent presentation to one bent upon learning the series is effective. In order that a stimulus may gain effectiveness for a response, the response must be the dominant activity or a part of the dominant activity of the moment. This fact is stated, not merely implied, in our wording of the requirement for gain of effectiveness. Again, the conventional statement in terms of contiguity of a conditioned and an unconditioned stimulus is ill-chosen, for in many cases the unconditioned stimulus for an activity cannot be specified, and yet the activity can be conditioned to specific stimuli. According to our statement, it makes no difference what the cause of the activity is. The important thing is that activity follows the stimulus and is dominant.

Since a stimulus gains effectiveness when followed by a dominant response, the problem of building up a desired effectiveness for a given stimulus is largely the problem of evoking the desired dominant activity after the stimulus is given. In a few cases a response may be aroused by application of a simple unconditioned stimulus. For example, the withdrawal or retreat responses are regularly elicited by noxious stimuli, and so the problem of developing a conditioned avoidance of any desired stimulus is merely the problem of presenting that stimulus along with some noxious stimulus. This fact may be briefly summarized as follows: *In order to evoke the activity which is desired, it is in some cases necessary only to present the unconditioned stimulus for that activity.* This statement approximates the conventional law of conditioning, but is placed here as a secondary law describing how the fundamental requirement for gain of effectiveness may be obtained.

As a general rule, however, the elicitation of most activities

depends upon internal conditions as well as external stimulation. Salivation and other responses involved in procuring, eating, and digesting food depend upon hunger, the instinct of food-getting, the desire for food, or what you will. The importance of these internal conditions for learning has been indicated by reference to instincts, drives, purposes, sets, motives, goals. The presence of a motive is not the fundamental requirement for learning, but rather it is a secondary requirement in obtaining the fundamental one. Thorndike has placed his law of readiness and his law of pleasurable affect among his primary laws. To the author these laws seem sound when placed as secondary and when the retroactive implications of the law of pleasurable affect are discounted. It is not the satisfying of hunger, a condition which comes after the act, that is essential to learning, but rather it is the hunger which is present to be satisfied and which predisposes the organism to vigorous actions of definite sorts that is important. The importance of these internal conditions is, however, not to be denied, and we shall indicate that importance in the following statement: *In order to evoke the desired activity, it is in most cases necessary to motivate the individual.* To motivate him is to arouse or take advantage of his desires, whatever the nature and origin of those desires. (The popular usage of the word desire is entirely satisfactory here.)

This examination of the conditions under which gain of effectiveness occurs shows one fundamental requirement and two methods of obtaining this requirement. The one method is stressed by those who set a high value upon the principle of conditioning and the other is emphasized by those who are impressed by the importance of motivation. Both groups are at fault in that they attempt to make a secondary condition primary.

Conditions for the Loss of Effectiveness by Stimuli

Opposed to the conditions which give an increased certainty that a stimulus is to be followed by a given activity are those which give a decreased certainty. It is the general

belief based on phenomena classed under the category of forgetting that a stimulus whose effectiveness has been acquired in the course of learning loses effectiveness when it is not presented to the organism for a period of time. This passive loss is described in Thorndike's law of disuse or in any of the usual statements of forgetting. Its validity does not concern us here, for the loss is not an outcome of active learning in which we are interested. The loss does not occur in the course of the organism's experience with the stimulation in question.

There is, however, a loss of effectiveness which is part and parcel of active learning, the loss which characterizes negative adaptation. The conditions under which this positive decrease occurs may be stated as follows: *A stimulus loses effectiveness for a particular response when that response does not occur upon application of the stimulus or when that response is superseded by an incompatible one while the stimulus is still present.* When flinching does not follow upon the application of an electrical shock, or when flinching in response to a shock is superseded by eating, the electrical shock loses effectiveness for causing flinching. When reaching to a candle flame is followed by vigorous retraction, the sight of the candle flame loses effectiveness for causing reaching. When the entering of a blind alley is superseded by leaving the alley, the sight of the alley loses effectiveness for causing entrance. When listening to a radio is followed by reading while the radio is still going, the sound of the radio loses effectiveness for causing listening.

Smith and Guthrie have made the most notable attempt to state the conditions under which stimuli lose in effectiveness.[3] If these conditions had been given more attention, there need never have been any great surprise that some frequently executed responses die out. Entering a blind alley may occur several times, but the mere fact of entering does not give an increased certainty of entering as many have supposed. It gives an increased certainty only if the entering is not superseded by leaving, and since entering a blind alley is followed

[3] Smith & Guthrie, General psychology in terms of behavior, chapter 3.

by leaving it, the more frequently it is entered the less, instead of the greater, is the likelihood that it will thereafter be entered. Learning not to do is as truly learning as is learning to do.

This active loss of effectiveness has been rather completely neglected in the discussion of the acquisition of skill. The problem has been frequently discussed as if the eliminated responses must be crowded out in some way by those which are not eliminated. For example, Watson has supposed that some responses because of their greater frequency or recency crowd out those which occur less frequently or recently, but under this supposition there is the unanswered mystery as to why there is not room for all. Hull has supposed that there is a sort of short-circuiting in the acquisition of skills, the stimulus for one response gaining so much effectiveness for a succeeding response that its own response is dropped out.[4] What takes place seems much more accurately described as an active elimination of certain responses, an elimination of those whose execution brings stimulation of a sort to evoke an incompatible response.

Since a stimulus loses its effectiveness when it fails to be followed by its customary response or when its customary response is superseded by an incompatible one, the problem of destroying effectiveness becomes the problem either of insuring the failure of the customary response or of making sure that the customary response is followed by an incompatible one. To insure the failure of the customary response, one may give the stimulus in a weak intensity or apply an inhibiting stimulus. Putting this in the form of a law we have the following: *A stimulus may be applied without causing its customary response if it is given in a weak intensity or if there is an inhibiting stimulus present.* This is a first secondary law for loss of effectiveness, and its truth is sufficiently recognized, I believe, to make comment unnecessary.

To make sure that the customary response is followed by an incompatible one, one may arrange conditions so that the

[4] C. L. Hull, Simple trial-and-error learning: a study in psychological theory, PSYCHOL. REV., 1930, 37, 241–256.

execution of the customary response brings into play the stimulation for the incompatible response, or so that the execution of the customary response does not remove the stimulation which evoked it. If a child customarily reaches to a particular object, his reaching will be superseded by vigorous retreat if the object is electrically charged or is heated. If a horse customarily bucks when a rider mounts him, the bucking will be followed by running, walking, or other activity if the rider is not dismounted by the bucking. Rewording slightly we have: *A stimulus may be followed by its customary response and then by an incompatible one if the execution of the customary response brings stimulation for the incompatible response or brings no relief from the original stimulation.* The loss of effectiveness when the execution of the customary response brings stimulation for an incompatible response, is described by Thorndike's law of unpleasant affect and in Carr's statements with reference to the role of sensory consequences in learning. These sensory consequences or unpleasant affects of acts are not the fundamental requirement for loss of effectiveness, but rather are important because they contribute to the fundamental requirement. True enough, what happens to a stimulus-response relationship, whether it is built up or torn down, depends upon what occurs after the response, but this does not mean a reversal of cause and effect. Stimulus A precedes both its customary response and the response which follows. It loses effectiveness for its own response A and gains effectiveness for the response B which follows. In so far as gain of effectiveness is concerned, there is simple conditioning of stimulus A to response B. This conditioning is, however, only half the picture. The other half is the dissociation of stimulus A from response A. This dissociation depends upon whether response A is or is not followed by an incompatible response. It may be further pointed out that whether response A is or is not followed by an incompatible response depends upon the reception of stimuli after A has been made. The importance of this fact is attested in Carr's law of sensory consequences and has been demonstrated in numerous studies in which knowledge of

results (stimulation after response) has been withheld, with the result that learning does not take place. Thorndike has reworded his law of effect so that it is no longer the old law of pleasant and unpleasant affects but rather a law pertaining to the knowledge of results. It might, perhaps, be better if we were to separate the two conditions under which a customary response is superseded by an incompatible one, so that the one condition would correspond to the law of knowledge of undesired or undesirable results.

This examination of the conditions under which loss of effectiveness takes place has revealed two essential conditions, both of which we have described in one statement. Each of these two fundamental conditions may be obtained in two ways, and so there are four ways in which a stimulus may be made to lose effectiveness for a given response. The one way which has received most attention is the presentation of stimulation for a response which supersedes and is incompatible with the customary response to the stimulus.

Summary

We have suggested that there are two fundamental laws of learning, one for gain and one for loss of effectiveness by stimuli for particular responses, and that there are secondary laws which describe the conditions necessary to obtaining the fundamental requirements. The suggested laws are as follows:

1. The law for gain of effectiveness. A stimulus gains effectiveness for a given response when the stimulus is followed by that response as a dominant response.

a. In order to evoke the dominant activity, it is in some cases necessary only to present the unconditioned stimulus for that activity.

b. In order to evoke the dominant activity, it is in most cases necessary to motivate the individual.

2. The law for loss of effectiveness. A stimulus loses effectiveness for a particular response when that response does not occur upon application of the stimulus or when that response is superseded by an incompatible one while the stimulus is still present.

a. A stimulus may be applied without causing its customary response if it is given in a weak intensity or if there is an inhibiting stimulus present.

b. A stimulus may be followed by its customary response and then by an incompatible one if the execution of the customary response brings stimulation for the incompatible response or brings no relief from the original stimulus.

This outline of the conditions required for learning has several significant differences from previous accounts.

(1) The first principle of conditioning is reworded in such a way as to avoid many misunderstandings arising from the conventional statement.

(2) It is pointed out that conditioning as ordinarily conceived applies to only a few instances of learning.

(3) Motivation is placed as a secondary rather than a primary requirement for learning.

(4) The facts described by Thorndike's law of effect and Carr's law of sensory consequences are recognized but are freed from the retroactive implications involved in Thorndike's and Carr's statements.

(5) Elimination of response in the acquisition of skills is conceived as an active elimination, as a positive loss of effectiveness by certain elements of the situation.

(6) Two reasons that frequency of stimulus-response relationship is often ineffective for causing learning are given. One is that the response may not be dominant and the other is that the response may be superseded by an incompatible one.

(7) There is no theorizing as to what connects a stimulus and a response. Only the stimulation and activity conditions for learning are dealt with.

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DISCUSSION

"PSYCHOGALVANIC REFLEX" NOMENCLATURE

Lauer[1] has recently classified and tabulated the titles of some three hundred articles bearing on the general subject of the psychogalvanic reflex. In his classification he finds that fifty-four definite terms have been used as part of the title of 185 of the articles. Of these fifty-four he shows that twelve have been used four times or more and that seventeen have been used three times or more. He points out that only eleven of these terms have a physical or biological meaning which is more or less definite. These eleven constitute thirty-four per cent of the one hundred and eighty-five classified titles, the remaining sixty-six per cent of the titles being non-specific. This lack of exactitude, coupled with the fact that the term 'psychogalvanic reflex' is in itself a mis-nomer, leads him to propose that the various phenomena usually dealt with under this heading be rechristened as the '*electro-bio-chemical response.*'

It seems to me that Lauer's analysis and proposal is open to certain points of criticism. In the first place his classification was made almost entirely on the basis of titles. It is a well known fact that the titles of articles are exceedingly general, and some will always be found which have only the haziest relationship to the subject matter of the paper. In the second place there is every reason to believe that a fairly large percentage of the titles which Lauer classified were only indirectly concerned with the 'psychogalvanic' literature. That is to say, any general bibliography on almost any scientific subject must of necessity include many titles of articles which pertain to the subject at hand only indirectly. Certainly this is true of the bibliography of Landis and De Wick on *The Electrical Phenomena of the Skin,*[2] whose list of three hundred and sixteen titles is the longest yet published. Lastly, the term *electro-bio-chemical response* is itself open to objection.

In order that we might examine the relationship existing between the titles employed and the subject matter of the articles, we tabu-

[1] A. R. Lauer, Why not re-christen the "psycho-galvanic reflex"? PSYCHOL. REV., 1931, 38, 369–374.

[2] C. Landis, & H. N. De Wick, The electrical phenomena of the skin (psychogalvanic reflex), PSYCHOL. BULL., 1929, 26, 64–119.

lated the contents of four hundred and forty-five articles which we have completely abstracted and which bear on the literature of the electrical phenomena of the skin. Of these, three hundred and sixteen were considered in the review of Landis and De Wick.[3] The remaining titles are to be cited in a review by the writer which is soon to appear in the PSYCHOLOGICAL BULLETIN. Adding together all *titles* containing the following phrases: psychogalvanic reflex; psychogalvanic phenomenon; galvanic skin reflex; psycho-galvanic reflex phenomenon; psycho-galvanic reaction; galvanic reflex; conditioned psychogalvanic reflex; and physico-psycho-galvanic reflex gave a total of 138 or 30.4 per cent of the titles. Adding together those articles which had been classified for *content* under the following headings: psychogalvanic reflex; applications of the psychogalvanic reflex; apparatus, methods or problems of the psychogalvanic reflex; psychogalvanic phenomenon; and physiological aspects of the psychogalvanic reflex gave a total of 158 or 26.8 per cent (the total for content entries was 591).

This indicates that in the bibliography on this subject approximately one-third of the work deals with the 'reflex' itself while the remainder deals with background or allied material. There is, of course, no reason to conclude that two-thirds of the articles are misnamed. A careful examination of all those articles in which the word 'psychogalvanic' was mentioned in the title failed to reveal a single instance in which the subject matter could be said to differ in its entirety from the implications of the title. In view of this it seems that our tabulations, as well as those of Lauer, do not justify any conclusion more sweeping than to say that there is a diversity of terminology used in the psychogalvanic literature and that many of these terms have no biological, chemical or physical, standing or meaning.

The crux of Lauer's argument (with which we are in essential agreement) lies in his point that the term 'psychogalvanic reflex' is in itself a mis-nomer and that it has been applied and used incorrectly in so many instances that it has led to marked scientific confusion and inexactness.

Veraguth[4] (pages 1 and 2), who first specifically named this phenomenon, accounts for the terminology as follows: "We suggest the name, psychophysical galvanic reflex phenomenon, or shorter,

[3] C. Landis & H. N. De Wick, The electrical phenomena of the skin (psychogalvanic reflex), PSYCHOL. BULL., 1929, 26, 64–119.

[4] O. Veraguth, Das Psychogalvanische Reflexphaenomen, Karger, Berlin, 1909.

psychogalvanic reflex, for the phenomena we are going to describe. Reflex phenomenon, because the oscillations of the mirror make it apparent that we deal with a phenomenon similar to the known reflexes . . . Psychophysical phenomenon, because the cooperation of psychic factors must be assumed as necessary to produce it. Galvanic reflex phenomenon, because we measure the outward expression of the phenomenon by means of a galvanic current."

In the light of present-day knowledge Veraguth's term is a misnomer. If 'psycho' implies mental or cerebral function, it is incorrectly used, since it has been shown that the phenomenon appears in a spinal preparation. 'Galvanic' is usually defined as referring to uninterrupted or continuous electrical current set up by certain chemical changes or actions. This phenomenon has been shown to be due to variations of a galvanic nature in the ionic permeability of the membranes of the sweat glands. Under the influence of nervous impulses the double-layer capacity of electrolytic dissociation, which exists at every living membrane, is altered. This alteration is shown by a change in the polarization-capacity of the membrane. Such alterations take place with extreme rapidity, probably within one ten-millionth of a second or less. The use of faradic or alternating currents of ordinary frequencies, which purport to do away with polarization, obviously does nothing of the sort. In short, the phenomenon is 'galvanic' but not in the sense in which Veraguth used the term or in the sense in which many subsequent writers have used it.

'Reflex' is sometimes defined as a simple, prompt, innate, motor reaction, usually mediated through lower nervous centers. This galvanic phenomenon is innate, but it is not simple; it is relatively slow; it is not motor in the sense that muscular or glandular reactions, *per se*, are solely involved; and it may or may not be mediated through lower nervous centers. In view of the looseness in the usage of the word 'reflex,' it would seem preferable to use the more general term 'response,' since the phenomenon is a response,[5] but it may not be a reflex. Such things as change in the true ohmic resistance of the body, changes in the apparent electrical resistance of the body, variations in the electrical potentials of the skin, chemical changes in the skin, changes in the permeability of the membranes of the sweat glands, changes in skin temperature, etc., etc., are all responses no matter how or by what electrical circuit they are demonstrated.

[5] Response may be defined as 'any change in the rate of energy output.'

During the past twenty years, a constant series of names has been advanced to cover the general phenomenon which Veraguth called the psychogalvanic reflex. Such names as the orectic reflex, the autonomic skin reflex, the electrical emotive reflex, the galvanic psychophysical reflex, the galvanopsycho reflex, the neurogalvanic response, the psycho-electro-tacho-gram, the 'so-called' psychogalvanic reflex, vasoconstrictor reflex, the emotive response, etc., etc., are found in the literature. To this list, Lauer proposes to add the term electro-bio-chemical response.

Just what points of real significance this new term of Lauer's adds to our terminology is not clear. The phenomena covered in this literature is so diverse that Lauer's term is as limited in its scope as most of the others. The only phrase actually covering the entire field which has been of particular interest to psychologists is *the electrical phenomena of the skin.* This term is general and has no specific implications. It designates a general subject in physiological psychology or physiology. This general term should be qualified when used, by stating the precise type of measurement or changes which are being studied. For example, such a phrase as 'the measurement of the electrical potentials of the skin in various mental conditions,' or 'variations in the apparent electrical resistance of the skin,' or again 'changes in true resistance brought about by variations in sympathetic innervation,' might be used.

I should like to submit the following definitions for terms relating to this field:

Electrical phenomena of the skin: Any change in the electrical properties of the skin as shown by any electrical measuring or indicating device. According to the best present-day knowledge, these changes are the end product of complicated neurological-, biological-, chemical processes.

Galvanic skin response: (1) Decreased apparent resistance of the skin due to physiological activity under the control of the autonomic nervous system, following sensory or ideational stimulation. (2) Increase in the apparent electromotive force of the skin similar to (1). [(1) called the phenomenon of Féré; (2) called the phenomenon of Tarchanoff. Both (1) and (2) are essentially sweat gland phenomena associated with the mechanism of bodily temperature control. Under certain conditions circulatory and trophic influences are important factors. Either (1) or (2) may be demonstrated by means of a galvanometer and usually are recorded by a photokymograph. The record is a wave-like curve, having a latent period of 3–5 secs. and a duration of 3–15 secs.]

Psychogalvanic reflex: An historical term used to describe the phenomena, defined under the term 'galvanic skin response,' but without the exactitude demanded by this definition.

In all other cases, I see no reason why the psychologist should not avail himself of the usual electrical and biological terminology.

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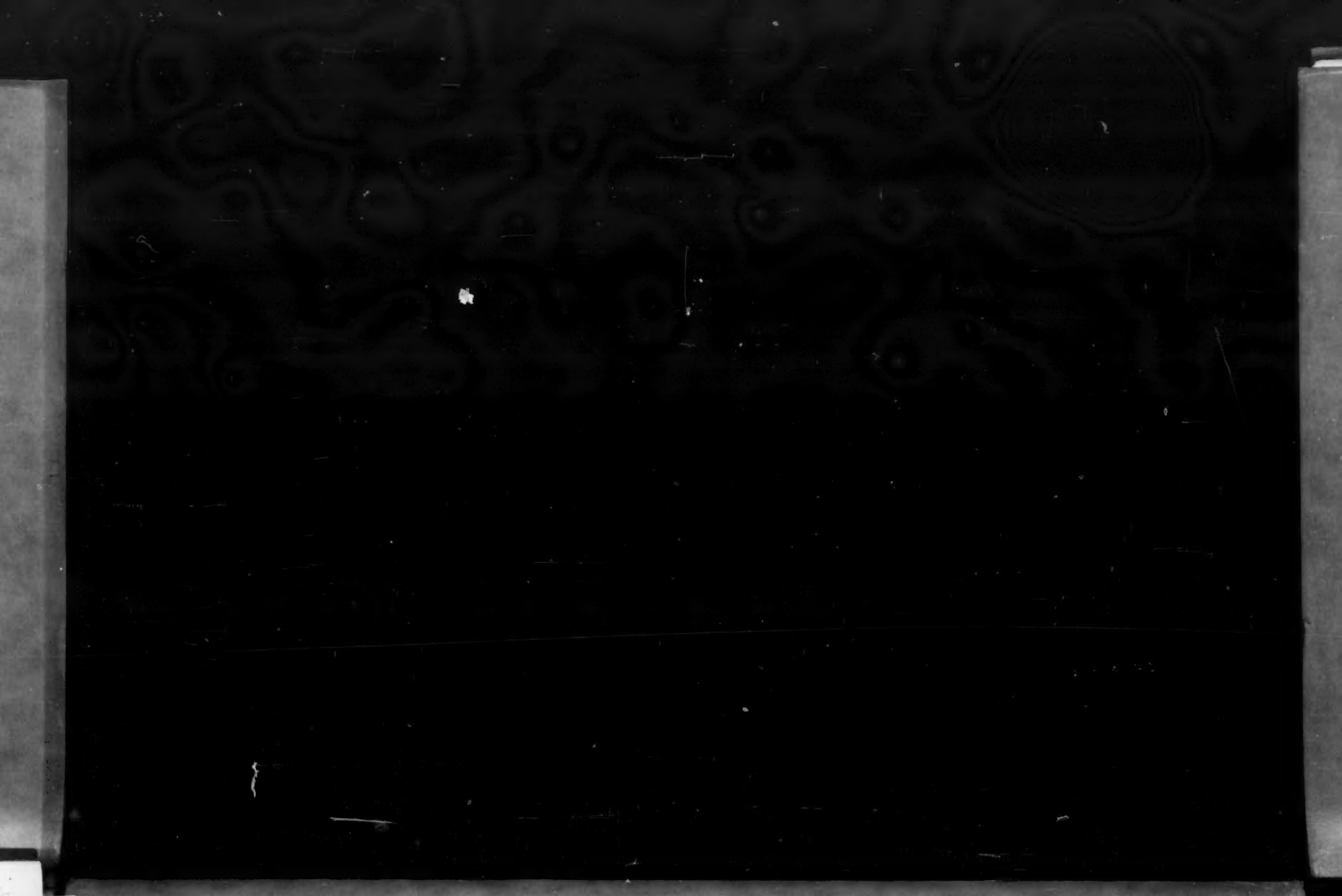
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